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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY
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AND
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“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit. lib. i. cap. l. Not.*

VOL. XXXVI.—FOURTH SERIES.
JULY—DECEMBER 1868.

L O N D O N.

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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.

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[FOURTH SERIES.]

JULY 1868.

I. *On the Dynamical Theory of Heat.*

By JOSEPH GILL, Esq.*

THE first law of thermodynamics enounces that heat and mechanical energy are mutually convertible. The direct conversion of mechanical energy into heat seems sufficiently obvious from various known phenomena of friction and percussion; and the admirable experiments of Joule have shown, with an accuracy sufficient for all practical purposes, that in the process of friction mechanical energy is directly converted or transformed into heat in the proportion of 772 foot-pounds for each British unit of heat. The direct conversion of heat into work is not so obvious, and satisfactory experimental proofs of the fact are still wanting.

Physically considered, the universe has been defined as matter and motion; and, as far as human means are concerned, it is allowed that matter and motion can neither be created nor destroyed. In all the wonderful transformations of matter revealed by chemistry, it is well known that the quantities of the substances acted on are absolutely constant; not an atom is destroyed or created; and physical science now demonstrates in a manner almost equally convincing that motion, or the free unbalanced action of force on matter, as well as force itself in a potential state, where it is counterbalanced by some equivalent resistance, is also absolutely constant in all the various transformations through which it may be caused to pass. The genius of Lavoisier established the truth of the conservation of matter; and many living authorities of the highest scientific standing have concurred in establishing the certainty of the conservation of

* Communicated by the Author.

force,—a theory which appears to shed a fresh light upon almost every branch of physical inquiry, and to impart a new interest to subjects the study of which was already beginning to settle down into a quiet current of fixed opinions.

The grand simplicity of the idea of the conservation of force, together with the tendency to generalize which influences most inquirers in scientific matters, would naturally lead to the belief that if mechanical energy is directly convertible into heat, heat should be also directly convertible into mechanical energy, and, of course, in the same proportion. Hence it is assumed that when a hot perfect gas performs work by expansion in a cylinder supposed to be neutral to the thermometric effects of heat, and having a piston moving without friction, it must naturally lose as much heat in raising a weight of 772 pounds one foot as would raise the temperature of one pound of liquid water 1 degree of Fahrenheit's scale. By the same reasoning, steam, in performing work in vessels supposed to be equally neutral to heat, should lose exactly the same quantity of heat in doing the same quantity of work; and as perfect steam (neither superheated nor containing water in suspension) cannot lose heat without undergoing a corresponding amount of condensation, it is assumed that in the working of the steam-engine as much steam is condensed (theoretically) as corresponds to the amount of work performed in the proportion of Joule's equivalent, and consequently that the quantitative heat of the steam passing into the condenser is by so much less than the total heat of the steam furnished by the boiler to the engine.

More than thirty years ago Seguin, in his interesting work 'On the Influence of Railways,' endeavoured to trace the dynamical relation which he saw must exist between the heat applied to the boiler and the energy developed by the engine; and it is remarkable that, in many experiments made by him on the actual working of steam-engines, with the express purpose of proving the disappearance of quantitative heat which he thought must be converted into the work done, he could not detect the expected disappearance of heat.

Soon afterwards Mayer published some very interesting speculations on the mechanical equivalent of heat; and, from theoretical considerations on the effect of heat in increasing the elastic pressure of air, he deduced a numerical value of the dynamical equivalent, which agrees very closely with the result of Dr. Joule's independent and highly philosophical experiments. These experiments proved indisputably that mechanical energy is directly convertible into heat; but so far no experiments seem to have proved with equal clearness that heat was directly convertible into mechanical work. More recently this part of the inquiry

was taken up by the distinguished engineer M. Hirn of Colmar, who, like M. Seguin at an earlier period, instituted a series of experiments on the actual working of large steam-engines, to ascertain, if possible, the disappearance of heat equivalent to the work done as assumed by the dynamical theory; but he could not obtain satisfactory proof of the direct disappearance of quantitative heat from the working steam as an equivalent of the work done. These negative results led to a controversy, in which the correctness of the dynamical theory was ably sustained by Professor Clausius; and at his suggestion M. Hirn made a fresh series of experiments on the large scale as before, from which he still could detect no disappearance of the heat supposed to be changed into the work done. His apparent facts were again ably combated by the supporters of the dynamical theory; and finally, in 1862, M. Hirn published a work in which he admits that his former views were erroneous, and shows himself a convert to the dynamical theory, giving a fresh set of experiments on large engines, in which he shows that heat does disappear from steam in the act of performing mechanical work. He argues that it must be so, as work or heat would be produced from nothing unless we allow that an equivalent of heat has been actually transformed into the work done, or reproduced in the heat developed by friction—and consequently that, in all cases where work has been performed by a hot elastic fluid, the fluid must contain less heat after the work is done than it contained before, the heat which has disappeared being the equivalent of the work done, and in fact transformed into this work. Moreover, when a gas is compressed it becomes hotter; when it expands against a moderated resistance its temperature falls. In a simply dynamical point of view, this case is like the winding up and unwinding of a spring: force from some exterior source is transferred to the compressed fluid, which in expanding back against a moderated resistance to its initial tension, gives out the same amount of force. But how explain the heating and cooling of the gas? Formerly the explanation was easy, on the supposition that the specific heat of gases varied with the density, the capacity for heat increasing as the density decreased. Regnault's experiments showed that the capacity of gases for heat is constant, or nearly so; consequently the facts of the heating and cooling of a gas by compression and expansion are inexplicable, and really without a cause, unless we allow a direct relation between the work expended or produced and the heating and cooling of the gas.

It has been supposed that M. Hirn's more recent experiments, described in his *Exposition Analytique et Expérimentale*, prove an exact proportionality between the work performed by the engine

and the excess of heat supposed to exist in the steam as it leaves the boiler above the quantity of heat which it contains as it leaves the cylinder, which latter quantity is found in the condenser—in other words, that the work performed is proportional to the heat which disappears from the steam in its passage through the engine. After careful examination of the phenomena, I think it will be found that this statement, though on the whole undoubtedly true, is not yet clearly established by direct experimental proof.

The quantity of work performed was ascertained by a friction-brake; but in these experiments it was of comparatively small importance to ascertain the exact quantity of work performed in each case, the object being rather to effect a constant amount of work with a variable consumption of steam. The tension of the steam in the boiler, its degree of superheat, and the quantity and temperature of the water injected into the condenser being all maintained constant, the external work performed by the engine may be modified in two distinct ways.

(1) This work may be diminished or increased by opening more or less the steam admission-cock.

(2) This cock being kept full open, and consequently a free passage of steam through it, the amount of work may be increased or diminished by cutting off the steam earlier or later during the stroke. Therefore, reciprocally, the work may be maintained constant if, while the amount of cut-off is diminished, the steam-admission is also diminished by “wire-drawing.”

By carefully clothing the cylinder, the loss of heat from exterior cooling may be reduced to a very small quantity. The proportion of work lost in friction and other resistances cannot be avoided to the same extent, nor is it easy to calculate it with even approximate exactness. But if the amount of work done by the engine is maintained constant while the consumption of steam is varied, as above mentioned, it may be supposed that the amount of work lost in friction &c. of the moving parts of the machine is also constant (or nearly so); so that the total quantity of external work performed by the steam may be supposed to be equal in each case. And as the consumption of steam to produce this constant amount of work may be caused to vary considerably (more than one-third in these experiments) by varying the mode of its admission to the cylinder, if still we find in each case that the *disappearance* of heat is the same in quantity, notwithstanding the variation in the quantity of steam consumed, it may be fairly deduced that the loss of heat in the steam is in all cases proportional to the amount of work performed.

To ascertain the amount of heat which disappears (or is changed into work) in each case, it is requisite, first, to know the quantity

of heat given to the steam before its admission to the cylinder, and, secondly, the quantity of heat given out by the steam in the condenser; the difference should obviously be the heat consumed in performing the work.

The quantity of heat required to evaporate a given weight of water under given conditions is known by Regnault's formulæ. Its value is complex; but this consideration does not greatly affect the present inquiry, as we are certain that the steam would give out in condensing all the heat it had previously absorbed for its formation, provided none of it be lost by any intermediate process—such, for instance, as the performance of external work at its expense.

In these recent experimental researches of M. Hirn, the steam on leaving the boiler was always more or less superheated, without change of pressure. This condition is indispensable for obtaining correct results, because ordinary steam generally carries into the cylinder a certain quantity of water, necessarily variable in amount, and very difficult to measure or estimate; consequently it is impossible in these circumstances to value correctly the quantity of heat furnished by the boiler.

The additional amount of heat required to superheat the steam a given number of degrees is also ascertained by a corresponding formula. We can thus ascertain the number of thermal units in the steam furnished to the cylinder in a given time; and, theoretically, this same quantity of heat should be found in the condenser, provided that none of it were lost during its passage through the engine by being changed into the work done.

The quantity of heat passing out from the engine in a given time can be easily ascertained by noting the quantity of water heated a certain number of degrees in the condenser during the same time. The difference between this quantity of heat and the total heat of the steam as it leaves the boiler in the same time should be the equivalent of the work done in that time.

In all these experiments the pressure in the boiler was maintained nearly constant, not varying more than one-tenth of an atmosphere; the temperature of the saturated steam was therefore known from the tables of tension. The temperature of the superheated steam was ascertained by a thermometer in the steam-pipe close to the cylinder, where the pressure was nearly the same as that in the boiler. The temperature of the condenser-water was taken every minute while the engine was in regular action, and was represented in each case by the mean of thirty observations.

To ascertain with accuracy the respective quantities of the steam and the injection-water employed in given times under the varying circumstances of the experiments was a task of great

difficulty. The quantity of steam furnished to the engine per second with the throttle-valve full open, and consequently the maximum amount of cut-off to produce the given constant amount of work, was ascertained by keeping the engine in constant action during a whole day; and the number of strokes made by the engine during this period was taken by a counter. By dividing the whole quantity of water by the whole number of strokes of the engine, the weight of steam for each stroke was ascertained.

It was much more difficult to ascertain the quantity of steam furnished to the engine in a given time while working without cut-off, and regulated to the same constant rate of work by throttling or "wire-drawing" the steam. In this case the consumption of steam, and consequently the consumption of fuel, increased; the furnace adapted for the more limited rate of combustion which sufficed for the more economical mode of working was not sufficient for the larger consumption; and under these circumstances the difficulty of maintaining the conditions of the supply of steam constant during a whole day's work was so great as to render it almost impossible to ascertain accurately by the former method the quantity of steam supplied; consequently another method was adopted for the purpose. The quantity of cold water supplied to the condenser, and the tepid water discharged from the air-pump in a given time, were each accurately gauged, the respective temperatures being taken into account; and the excess of the latter quantity over the former, representing the steam passing through the engine, being divided by the number of strokes in the given time, showed the weight of steam used per stroke.

The first series of experiments described by M. Hirn were made with a large engine making twenty-seven strokes or revolutions per minute, and producing the constant work of 150 horse-power, as shown by a friction-brake. Under the most economical conditions of the action of the engine this work was produced from k. 0.34554 of steam per second, representing 228.16 French thermal units or calories; and under the least economical conditions the steam used was k. 0.46927 per second, equal to 286.38 calories. In the former case a disappearance of 40.34 calories was observed; and in the latter case heat equal to 38.08 calories disappeared.

A second set of experiments with the same engine, producing work equal to 116 horse-power, showed a consumption of k. 0.23548 of steam per second in one case, and k. 0.32307 in another—the observed loss of heat being 30.51 calories in the first case, and 32.13 calories in the second case.

Another set of experiments with a smaller engine, making

ninety-three revolutions per minute, and producing the constant work of 90 horse-power, showed the steam used to be k. 0.1902 per second in one case, with an observed loss of heat equal to 21.5 calories; and in another case the consumption of steam was k. 0.229 per second, with a loss of heat equal to 22.58 calories.

In the most economical of the above experiments a constant amount of work equal to 116 horse-power was obtained from k. 0.23548 of steam per second, the steam, of five atmospheres' pressure, being superheated 93° C. (from 152° to 245°) and cut off early, so that the expansion was as 6:1. In another experiment, the steam being equally superheated and cut off near the end of the stroke, so that the expansion was only as 1.15:1, the rate of work being kept constant by throttling or wire-drawing the steam, the same 116 horse-power required the consumption of k. 0.32307 of steam per second, being 36 per cent. more than in the former case. Now, everything else being equal, the furnace must furnish in the second case one-third more heat than in the first case. If, therefore, reasons M. Hirn, we find in the condenser-water also one-third more heat, we may fairly deduce that the amount of heat consumed in its passage through the engine depends entirely on the quantity of external work produced; for by this mode of experimenting, the quantity of work obtained being constantly 116 horse-power, the proportion of loss from friction &c. should be the same in all cases, and therefore need not be calculated. Now the heat which disappeared was nearly equal in both experiments, being 30.51 calories in the first case, and 32.13 in the second; and the mean of all the experiments showed a still nearer approach to equality. It was therefore deduced that in all cases an exact proportionality exists between the work produced by the engine and the difference between the quantity of heat furnished before the steam enters the cylinder and the quantity of heat remaining in the steam as it leaves the cylinder.

As a result of my own experiments, and from what I have understood of the experimental investigations of Seguin and of Hirn on the working of steam-engines, I have for many years entertained the opinion that the *direct* change of heat into work has not been satisfactorily shown by experiment; and it still appears to me that when the heat employed in working the engine is supposed to be merely the heat of the steam as it enters the cylinder (whether saturated or superheated), and *when the steam works at full pressure throughout the whole stroke*, none of this heat should disappear theoretically in the production of work, as in this case all the heat of the steam which passes through the cylinder should be found in the condenser. Con-

sequently, as regards tangible proof from direct experiment, M. Hirn's earlier opinion that heat did not disappear from the steam in the production of work may have been really in accordance with facts as far as they were perceived, though at variance with the whole truth; and I would submit that the conclusions arrived at from his more recent experiments, as above quoted, though in accordance with truth, may be questioned as to their satisfactory proof by experiment. In short, I now imagine that our investigations on the subject by experiments on the actual working of steam-engines have not been conducted on really correct grounds, and that the results have therefore been anomalous. It is no doubt true that the heat furnished by the fire is in all cases more than the heat which passes into the condenser; and if no heat were otherwise lost, this excess would correspond to Joule's equivalent; but when the engine works with the full boiler-pressure throughout the whole stroke, it seems evident that the whole work is in effect done in the boiler by the expansion of the water into steam, and is merely transmitted by the steam to the piston; so that the heat equivalent to the work done disappears from the boiler, and is locally made up by the fire directly without any indication of these phenomena being perceptible in the subsequent parts of the process of working the engine. If this view of the subject is correct, it is difficult to perceive how M. Hirn's deduction of an exact proportionality between heat lost and work done could be fairly proved from the experiments (though doubtless the law is true); for with a cut-off at one-sixth of the stroke, about two-thirds of the work would be done by the expansion of the isolated steam in the cylinder, with a corresponding disappearance of heat in the condenser, which should be very perceptible; while in proportion as the engine worked with steam approaching to full pressure, the proportion of work actually done by the steam in the cylinder would become less and less, until the disappearance of heat from the working steam might become inappreciable.

In order to maintain the work of the engine constant with a variable consumption of steam, throttling or wire-drawing was used more as the expansion from cut-off became less; and as the process of wire-drawing increases the heat of the working steam *at the expense of the heat in the boiler*, the quantity of heat passing into the condenser would be affected also by this cause.

It is certain that the performance of work by the expansion of isolated steam drawing solely on its own self-contained sources of energy must cost to the steam a full equivalent of force in some shape; and as in this case no other form of force can be directly detected except heat (or molecular motion, as it is now

defined), and it is mathematically demonstrated that heat should disappear in such cases and might be transformed into the work done, it may be freely admitted that the work performed by the expansion of isolated steam is accompanied by a corresponding disappearance of heat. This disappearance of heat, if the steam is saturated, should cause a corresponding condensation of part of the mass, or an equivalent amount of cooling if the steam is sufficiently superheated to bear abstraction of heat without condensing. If a reservoir of saturated steam be put in free communication with a cylinder and loaded piston, the piston's outward motion would be accompanied by an expansion of the whole mass of steam, and a condensation of steam-particles throughout the mass equivalent to the amount of work done by the piston. If, now (the communication with the working cylinder being closed), fresh steam from some other source be forced into the reservoir until its original pressure is recovered, the compression thus effected should reevaporate the liquid particles of condensed steam, and the general mass of steam should become again dry, or merely saturated. If yet a further quantity of steam equal to a stroke of the working piston be injected into the reservoir, the mass of steam should become superheated; and the fall of temperature and pressure consequent on the succeeding stroke would now bring back the steam to its initial state of saturation. Thus it would seem that heat should equally disappear from an isolated mass of steam doing work by expansion, whether the steam be saturated or superheated; but in the case here imagined the disappearance of heat would be from the whole mass in the reservoir and cylinder together.

In the circumstances above imagined, the source of a continuance of work would be the injection of a continued supply of steam into the reservoir; and the operations of injecting and withdrawing steam being intermittent and occurring at alternating intervals, a corresponding fluctuation of density and temperature would occur in the whole mass, with periodic superheating or condensation, as the case might be, but without permanent disappearance of heat except in the cylinder, the contents of which formed part of the general mass during the working stroke, and participated in the general loss of heat assumed to be transformed into the work done. Thus, if the cylinder be $\frac{1}{10}$ of the volume of the reservoir, the disappearance of heat from the cylinder itself would be $\frac{1}{10}$ of the whole heat supposed to disappear; the remaining $\frac{9}{10}$ would be replaced in the reservoir by the succeeding stroke of the injecting pump, while the contents of the cylinder would be discharged into the condenser in the state in which the fluid remained at the end of the working stroke.

If the steam supplied to the reservoir were injected at the same time and at the same rate as the steam is withdrawn in giving motion to the working piston, so that the density and pressure in the reservoir should remain constant, it would appear that the whole work would be done directly by the injecting piston, and merely transferred to the working piston by means of a moveable plug of steam at constant pressure and temperature. In this case no heat should disappear either from the reservoir or from the cylinder; and the contents of the cylinder at the end of the working stroke should be identical with an equal mass of steam of corresponding pressure and temperature from any common source.

The formation of steam from the water in a boiler may be considered equivalent to the injection of steam ready formed from some other source; and during the whole stroke, if at full pressure, or the full-pressure part of the stroke if working expansively, only a small fraction of the heat assumed to be converted into work would disappear from the cylinder itself, being in the proportion of the contents of the cylinder to the whole contents of the steam-space in the boiler, and smaller as the fluctuations of pressure in the boiler due to the intermittence of supply to the cylinder are bounded by narrower limits. On the other hand, all the loss of heat which corresponds to the work done by expansion in the cylinder after the cut-off should take place in the cylinder itself. Hence, in experimental researches on the assumed disappearance of heat from the steam in the act of doing external work, we should not expect to find tangible proof of this phenomenon by comparing the quantity of heat in the steam working at full pressure in the cylinder during the whole stroke with the amount of heat found in the condenser; for theoretically these quantities may be nearly equal, if, as supposed above, the work given out by the piston is virtually done in the boiler; and the steam which fills the cylinder at the end of a working stroke may differ but slightly from an equal weight of common saturated steam of the same pressure taken directly from a boiler. If such be the case, it were useless to search *in this part of the process* for a loss of heat equivalent to the work done; for the exhaust-steam dashing tumultuously into the vacuous condenser should produce there as much heat as there is cold produced in the cylinder by the expansion which drives out the steam—as in Joule's experiment of causing compressed air to expand into a vacuous receiver, by which process the total quantity of heat remains unaltered. This view of the case may help to account for the apparently anomalous results obtained by Seguin and by Hirn in their researches on this subject; and as in my own experiments sufficient account was not taken of the influence of

expansion from cut-off, I think I can now detect sufficient cause why I could not discover the disappearance of heat which might be fairly attributable to a transformation into the work done by the engine.

On the other hand, if an isolated mass of steam, for example the steam which fills the full-pressure portion of the stroke at the moment of cut-off, be caused to produce work by its own expansion during the remainder of the stroke, it must lose something equivalent to this work—apparently a proportional quantity of its own heat; and as saturated steam cannot part with any of its heat without suffering a condensation of some of its particles, it should follow that the moderated expansion of steam in the act of doing work should be accompanied by the condensation of steam-particles throughout the mass, representing an aggregate amount of heat equivalent to the work done. Thus the steam working expansively in the cylinder isolated from exterior sources of heat or cold, should assume the condition of a mist from the formation of numerous minute water-particles throughout its mass; and as the heat which had previously maintained these particles in the state of vapour no longer exists in the cylinder, having, in fact, been transferred or transformed into the work done by the expansion, the remaining mass of expanded steam must contain so much less heat, and should accordingly show less in the condenser.

The grand principle of the Conservation of Energy indicated with indisputable certainty that the work done by a steam-engine must cost its full equivalent to the fire; but in the absence of any direct proof of the disappearance of this equivalent of heat from the steam in its passage through the engine, I was fain to suppose that the heat in passing into the boiler might undergo a transformation into some form of repulsion in the vapour-particles corresponding in intensity to the temperature and pressure of the steam—and that the work done might be attributed to an equivalent disappearance of this repulsion with a fall of temperature and tension in the steam, but without loss of its quantitative heat, considered simply as molecular motion. It will perhaps be allowed that this hypothesis was not altogether unreasonable so long as there was no satisfactory experimental proof of the direct disappearance of heat from the steam in its passage through the engine; and even if I had proved this disappearance of heat, I might still have remained in doubt as to the mode of the change of heat into work so long as I was uncertain whether this process might take place simply on mechanical principles applied to molecular action, as shown in Professor Rankine's hypothesis of molecular vortices. This uncertainty has been removed by the complete confirmation of the results of Regnault's

experiments, showing a constant specific heat of air at different densities which I have recently obtained from a repetition of some of his experiments in a different shape; and I hope to be able soon to complete some experiments which should furnish additional proof of the actual disappearance of heat in the production of work in the steam-engine. The want of such direct proof is acknowledged; and its importance is evident when we consider how intimate is the connexion between a more extensive knowledge of this subject in a practical point of view and the improvements to be made in the wide field which still remains open for effecting economy of fuel in the working of our thermic prime movers.

Palermo, April 10, 1868.

II. *On Vortex-rings in Air.*
By Professor ROBERT BALL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE production of vortex-rings in air is experimentally shown by the interesting phenomena produced by Professor Tait. These are described by Sir William Thomson in the *Philosophical Magazine* for July 1867. I repeated these experiments with the kind aid of Dr. Emerson Reynolds; in doing so an interesting variation of them presented itself which renders the air-rings actually visible.

While engaged on this subject, Mr. Yeates suggested to me to try the effect of discharging an air-ring at a column of smoke; this was done, and a most curious and unexpected appearance was the result. The box employed was a 2-foot cube, and had a hole 8 inches in diameter, the side opposite having a piece of stout sacking strained over it. A blow on the sacking causes an air-ring to dart from the hole. Two large flasks containing respectively hydrochloric acid and ammonia were arranged with their mouths in close proximity and their contents vigorously boiled; from the union of their vapours a dense column of the fumes of chloride of ammonium ascended. This apparatus was placed about eight or ten feet in front of the box. An air-ring on first leaving the box was of course invisible; when it reached the column of smoke it could be seen to force its way through it; but when it left the column and during its subsequent existence its appearance was very remarkable. At first the idea suggested itself that the single air-ring had formed two concentric smoke-rings; but closer examination and an improved column of smoke showed what had really occurred. The air-ring had penetrated

the smoke quite uninjured ; it had not apparently left any of its particles behind, nor had it admitted an atom of smoke into it ; but it had drawn with it sufficient of the smoke to form a complete shell, which enclosed it, and thus rendered the air visible. The phenomenon is quite in accordance with that conservative tendency which theory shows must belong to a vortex-filament. The appearance is one of great beauty, and suggested the name "negative smoke-ring."

It was considered desirable to make these experiments before an evening scientific meeting of the Royal Dublin Society. Smoke-rings are seen to greatest perfection when directed along a sunbeam admitted into an otherwise darkened room ; and though they can be seen beautifully in ordinary diffused sunlight, by gaslight they make a very poor show ; special means of illumination are therefore necessary. The following arrangement is found to succeed admirably. By a simple optical contrivance the radiation from a brilliant lime-light is concentrated into a slightly conical beam, all other light in the room being extinguished. The box is placed in the beam, the orifice facing the lamp, and from twenty to thirty feet distant from it ; at the box the diameter of the circular section of the luminous cone is from 3 to 4 feet. A smoke-ring driven from the box will, if suitably directed, traverse the beam through its entire length until it reaches the lamp ; and as it is brilliantly illuminated throughout its path, the appearance presented is of great beauty. A second box can be placed at the lamp end of the beam, so as to show the effect of the collision of rings. The fumes produced by burning a small piece of phosphorus developes an ample supply of smoke in the interior of the box. This mode of producing smoke was suggested to me by Dr. William Barker ; it is simpler to manage inside the box than the apparatus necessary for forming chloride of ammonium. By the introduction of coloured glasses various pleasing effects can be produced. If a sheet of tissue-paper be very lightly attached to a frame and interposed in the beam, a vigorously sent smoke-ring will sweep it away in a striking manner.

To exhibit the air-rings, the column of smoke already described is placed so as to ascend through the beam, and rather nearer the box than the lamp. An air-ring from the box is, of course, invisible till it reaches the column, while in its passage from the column to the lamp the curious phenomenon already mentioned is most beautifully shown. The negative smoke-rings are much better seen when thus illuminated than by ordinary daylight. A box of the dimensions previously given, viz. a 2-foot cube with a hole of 8 inches diameter, was found to answer better for this purpose than a somewhat smaller box with a hole of 6 inches aperture.

The column of smoke may perhaps serve as a test for the existence of vortex-rings in other cases, when, produced in air, they are invisible. This was tried in one instance. It is a common trick to blow out a candle by the puff of air from the muzzle of a gun when a percussion-cap is exploded on the nipple. This puff is probably a vortex-ring. Owing to the high velocity with which it moves, it is difficult to trace the effect produced by the column of smoke. One person, however, firing from an elevated position and at a distance of fifteen or twenty feet from the flasks down upon the column, and another watching against a dark background, a distinct ring was occasionally seen to dart from the column after the percussion of a cap; but whether these were "negative rings," as was expected, or ordinary "smoke-rings," was not easy to determine.

I remain, Gentlemen,

Yours &c.,

Royal College of Science for Ireland,
Dublin, May 28, 1868.

ROBERT BALL.

P.S.—Since writing the foregoing, it occurred to me to fill the box with ammoniacal gas and to discharge rings from this at a column of the vapour of hydrochloric acid. In this case the ring, before reaching the column, is perfectly invisible, and the existence of the column is only seen by slight traces of partially condensed vapour. As was expected, a beautiful ring appeared, from the combination of the two gases, when the ammoniacal ring reached the column.

June 16, 1868.

III. *On C. S. Lyman's new form of Wave-apparatus*.*

THE theory of waves that has been generally taught since the days of Newton is that which represents wave-motion as consisting in the alternate rising and falling of the parts of a liquid in vertical lines, as in the two branches of a U-shaped tube; this is usually cited as Newton's theory of waves. There is to be found, indeed, in the *Principia* the hypothesis of vertical oscillations, and also the cut of the bent tube, so persistently copied by subsequent writers; yet it is evident that Newton adopted the hypothesis rather as an expedient for a special purpose (that of finding approximately the relation of a wave's length to its period) than as strictly true to nature; for he concludes his investigation with the remark, "These things are true, upon the supposition that the parts of water ascend or descend in a right line; but, in truth, that ascent and descent is rather performed in a circle [*verius fit per circumlum*]; and there-

* From Silliman's American Journal for January 1867.

fore I propose the time defined by this proposition as only near the truth." This important qualifying clause seems to have been quite overlooked by those who have professed to give Newton's theory of waves.

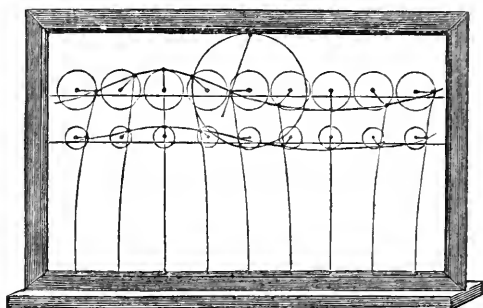
The modern theory, which teaches that in wave-motion all the particles of a liquid are revolving synchronously in vertical circles, though so broadly hinted at by Newton himself as the true one, in the words italicized above, has nevertheless been developed for the most part only within the present century. It was first clearly stated and ably advocated by Gerstner in 1804. More recently it has been satisfactorily established as accordant with observed facts and the laws of mechanics, by the experimental researches of Weber and Scott Russell, and the mathematical investigations of Stokes, Rankine, and others. A concise exposition and demonstration of the theory may be found in a paper by Professor Rankine, in the *Philosophical Transactions* for 1863. Though but just beginning to find its way sparingly into the text-books, it has become the generally accepted theory among men of science; and having in reality supplanted the old hypothesis as an expression of scientific truth, there is no good reason why it should not also supplant it in the lecture-room.

In teaching this theory, however, it is often found difficult to make pupils understand how the infinitude of simultaneous revolutions, which it supposes, can take place without mutual interference, and in such a way as to produce the observed phenomena. It was to obviate this difficulty, and illustrate, as far as practicable, the *modus operandi* in question, that the piece of mechanism was devised which is the subject of this article. It presents to the eye not merely the surface contour of a wave with its undulatory motion (which to some extent other forms of wave-apparatus also do), but, besides doing this in a more exact and satisfactory manner, it exhibits the motions also that are at the same time taking place below the surface in the whole mass of liquid affected. This completeness of illustration is due to the circumstance that in the apparatus the elementary motions are the same essentially as in actual waves; hence the leading points, geometrical and dynamical, of the theory of waves are presented naturally and in their true relations.

The construction of the apparatus will be readily understood from a brief description and the accompanying woodcut.

In front of a plane surface are two series of revolving arms or cranks, the length of the lower ones being half that of the upper. Two elastic wires connect the crank-pins of each series; upright wires also connect each pair of cranks, and pass down through a plate into the base. The cranks all revolve synchronously; they thus keep their relative position, and come into any given position

successively, each in its turn. The relative position of the cranks of each horizontal series is such that the directions of any two, in



regular order, differ by the same fraction of a whole revolution that the distance between their axes is of a whole wave-length. Thus in the apparatus the wave-length is supposed to be divided into eight equal parts; and hence the common difference between the directions of adjacent crank-arms is one-eighth of a circle, as shown in the figure. The cranks in each vertical set have their positions always alike. The number of cranks, whether taken horizontally or vertically, is arbitrary—a matter of convenience in construction. The synchronous revolution of the cranks is effected by means of any suitable mechanism—such as equal toothed wheels on the several axes, with alternate idle wheels connecting them; or equal rag-wheels, with endless chain or metallic ribbon; or equal cranks, with a rigid connecting frame or plate. The first method is used in the original machine, the third in the model for the Patent-Office, the second and third in the larger and smaller sizes respectively for the market.

The crank-pins represent as many liquid particles, the circles on the background their orbits. The transverse wires represent continuous lines of particles, which at rest would be horizontal, and be represented by the lines on the background drawn just below the centres of the orbits—the upper one of these being the surface-line, the lower a line of particles one-ninth of a wave's length down. The upright wires represent lines of particles which at rest would be vertical. Every point in these moving lines describes its own distinct orbit.

The apparatus is constructed to a scale, and so represents a wave of given length, height, and period; but equally represents also a wave of any other length and proportionate height, though of period proper to its length, according to the law of that relation, as stated further on. In the original instrument, for example, the wave-length is 36 inches, height from trough to crest 4 inches, and period for that length 0.76 second; but it

equally represents a wave whose length is 36 feet and height 4 feet, with period 2.63; and similarly for other proportional dimensions.

Among the particular points in wave-phenomena which are elucidated by this apparatus may be enumerated the following:—

1. *The undulating surface-profile*.—This is shown in the motion of the upper flexible wire, which presents a continuous contour-line, of the exact curvature throughout of a true normal wave—instead of a broken contour of arbitrary form, by means of rising and falling balls, as in the ordinary wave-apparatus.

2. *The undulatory motion of all subprofiles, or lines of equal pressure down to still water*.—The representative of such lines is the lower transverse wire, which moves similarly to the upper one, but with a less curvature. Every such line of equal pressure is a continuous one, composed of particles in a state of dynamical equilibrium, and constituting an ideal moving wave exactly as if at the surface, the corresponding phases of all such waves being on vertical lines.

3. *The genesis of the undulatory motion from the circular motion of revolution*.—This is seen in the mode in which the crank-pins, in each transverse series, or the particles which they represent, come in regular succession into a given position as they revolve synchronously in their orbits.

4. *The equality of the height of a wave from trough to crest, with the diameter of the orbits of the surface particles*.—This is obvious in the apparatus, and follows directly from the mode in which the wave-surface is generated.

5. *The direction of motion of particles of water in the different phases of a wave*.—A glance at the motion of the crank-pins shows that a particle at the wave's crest is moving forward, or in the direction in which the wave is propagated, and a particle at the trough in the reverse direction, or backward, that a particle on the forward slope is rising, and one on the back slope descending. The same is true of particles in all the subwaves, or surfaces of equal pressure, down to still water.

6. *The length of a pendulum keeping time with the wave*.—This is equal to the radius of a circle whose circumference is the wave's length. Such a circle is the large one drawn on the background, as shown in the figure. Its radius is to that of a particle's orbit (or length of a crank-arm) as the particle's weight is to its centrifugal force. Or putting R and r for these radii respectively, and t for the time of revolution, we make

$$R : r :: g : \frac{4\pi^2 r}{t^2},$$

whence

$$t = 2\pi \sqrt{\frac{R}{g}};$$

which is the period of a revolving pendulum, or the time of a double oscillation of a simple pendulum whose height is R . Compare (10).

7. *The dependence of a wave's period on its length alone, not on its height.*—This is a corollary from the preceding. The period varies as the square root of the length, and is the same for all subwaves as for the surface-wave, the length being the same for all. The height, within certain limits, is independent of the length, as appears in the apparatus, and as may be inferred from the formulæ given further on. It depends on the centrifugal force of the particle, and this ultimately on the external forces generating it.

8. *The varying direction and intensity of the resultant force acting at each instant on a given particle in a wave.*—The component forces are two—the particle's gravity and its centrifugal force. The former is represented by the vertical radius of the large circle, the latter by the radius vector of the revolving particle; their resultant, then, is represented by the third side of the triangle of forces, or the side formed by the wire pendulum. This resultant must be always normal to the wave-surface, as the wire pendulum is seen to be always at right angles to the elastic wire representing that surface.

9. *The condition of a wave's rupture at the crest.*—When the centrifugal force becomes equal to gravity (or the radius of the orbit to that of the large circle), the resultant force for a particle at the highest point of its orbit or crest of the wave must be zero, and the particle consequently fly from its orbit, or the crest break in foam.

10. *The trochoidal form of the wave-curve.*—The point of suspension of the pendulum, that is, the upper extremity of the vertical radius of the large circle, may be regarded as the instantaneous centre about which an element of the wave-curve at the point of normality of the pendulum is described. Consequently, if this circle be rolled under a horizontal straight line, a point within it distant half the height of a wave from the centre will trace the wave-profile, which therefore is a trochoid. The rolling circle is the same for all wave-profiles down to still water, the lengths of the tracing-arm only differing. The circumference of this circle equals, of course, the wave's length.

11. *The greater sharpness of the crests than of the troughs of waves.*—This follows from the preceding, and is shown in the relative positions of the crank-pins—nearer together at the crests, further apart in the troughs. The trochoids become necessarily sharper at the upper bend, and less so at the lower, as the tracing-arm approaches to an equality with the radius of the rolling circle; until, when that equality occurs, the trochoid

passes into the cycloid, which has sharp cusps. The cusp of the inverted cycloid, then, is the limit of sharpness of a wave's crest. The equality above named is equivalent to that of the centrifugal force of a particle with its gravity (9). When the latter condition occurs, the wave-curve is cycloidal, and only then.

12. *The limits of possible curvature of waves.*—That curvature must always lie between the cycloid at the one extreme, and the straight line at the other, embracing trochoids of every possible variety.

13. *The greater elevation of the crests above the level of still water, than depression of the troughs below it.*—The difference between this elevation and depression is equal to twice the height due to the orbital velocity of the particles—that is, to twice the height from which a body must fall to acquire that velocity—or is a third proportional to the radius of the rolling circle and that of the particle's orbit; that is, putting R and r for these radii respectively, v for the orbital velocity $\left(= \frac{2\pi r}{t}\right)$, and D for the difference in question,

$$D = \frac{r^2}{R} = \frac{v^2}{g}.$$

When r equals R , then $D = r$, or half the height of the wave.

14. *The elevation of the centres of the orbits of particles above the positions of the same particles at rest.*—This is shown in the distance of the axes above the corresponding lines on the background. These lines show the positions of lines of particles at rest, which in motion form the wave-profiles represented by the transverse wires. The elevation in question is equal to the height due to the particle's orbital velocity, or is a third proportional to the diameter of the rolling circle and the radius of the orbit, or is equal to the area of the orbit divided by the length of the wave; that is, putting H for this elevation, l for the wave's length, and the other symbols as before,

$$H = \frac{r^2}{2R} = \frac{v^2}{2g} = \frac{\pi r^2}{l}.$$

When r equals R , $H = \frac{r}{2}$, or one-fourth the height of the wave.

To this elevation is due one-half the mechanical energy of a wave, the other half to the motion of its particles. That energy is, in other words, half potential, half actual.

15. *The decreasing diameter of the orbits with depth.*—This is seen in the shorter crank-arms below, and the decreasing amplitude of sway of the upright elastic wires down to their points of rest, which mark the depth of still water. The decrease of the orbits in diameter takes place in a geometrical ratio, and is ap-

proximately one-half for each increase of depth equal to one-ninth of a wave-length; or more exactly, putting r and r' for the radii respectively of a surface-orbit and of one whose middle depth is k , it is

$$r' = re^{-\frac{k}{R}},$$

R being, as before, the radius of the rolling circle, and e the base of the Napierian logarithms.

16. *The peculiar swaying motion of continuous lines of particles of equal pressure which at rest are vertical.*—These lines are alternately lengthened and shortened, and bent to right and left, as represented by the upright elastic wires.

17. *The varying distortions undergone by blocks or sections of water originally rectangular, or rectangular when at rest.*—Such sections are represented by the spaces between the wires, and their distortions by the distortions of these spaces.

18. *The fact of sensibly still water at half a wave's length below the surface.*—This is exhibited in the absence of lateral motion at the lower extremities of the upright wires, and is a necessary result of the law of diminution of orbits with depth, as given above (15).

19. *The varying strain in wave action on floating bodies.*—This is seen in the varying angle made by the upright wires with the upper transverse wire: the latter shows the position of a raft, for example, lying on the wave-surface; the former, that of a long thin body, as a board floating end down; hence the varying relative direction of the wires shows the strain to which a body is subjected, having both breadth and depth, as the hull of a vessel.

Many other points besides the above may be studied to advantage in connexion with this apparatus, but it is not important to specify them here. Enough has been stated to illustrate its utility, and indicate in what respects it differs from every other form of wave-apparatus.

For convenience of reference and for the sake of completeness, a few formulæ are added, expressing other relations among wave-phenomena not so directly exhibited by the instrument, but important to be presented in connexion with it. Putting V for the velocity of propagation of a wave, and the other symbols as before, the length of the wave is

$$l = 2\pi R = \frac{gt^2}{2\pi} = \frac{2\pi V^2}{g} = tV;$$

its period

$$t = \sqrt{\frac{2\pi l}{g}} = 2\pi \sqrt{\frac{R}{g}} = \frac{l}{V};$$

the velocity of a particle in its orbit, or at the crest of the wave,

$$v = \frac{2\pi r}{t} = r \sqrt{\frac{g}{R}} = r \sqrt{\frac{2\pi g}{l}} = \frac{Vr}{R} = \frac{2\pi r V}{l} = \frac{gr}{V} = \frac{tgr}{l};$$

the velocity of propagation of the wave

$$V = \frac{l}{t} = \frac{gt}{2\pi} = \sqrt{\frac{gl}{2\pi}} = \sqrt{gR} = \frac{gr}{v};$$

the sine of the angle of steepest slope of surface is

$$\sin \theta = \frac{2\pi r}{l} = \frac{r}{R}.$$

It will be understood that the normal wave, to which the theory applies, and which the apparatus illustrates, is the wave on deep water, or water a wave's length at least in depth. In shallow water the orbits are no longer circles, but ovals, or approximate ellipses of less height than length, according to the degree of shallowness.

When waves pass from deep into shallow water, as toward a beach, they become gradually shorter, their total energy is imparted to a less and less mass of liquid, and the extent of the motion of the particles is proportionately increased. The crests also travel faster than the troughs; so that the front of each wave becomes by degrees steeper than the back, and at length curls forward and falls over, exhibiting the well-known roll of surf. The formulæ for waves in deep water, therefore, require modification to adapt them to waves in shallow water, where depth of liquid and ellipticity of orbit enter as elements.

It has been necessary, in order properly to explain the apparatus and its uses, to give more fully the leading points of the theory of waves than would be required, were the works containing it more generally accessible. For these points the papers of Professor Rankine have been chiefly consulted. It is hoped that this outline of the theory, thus incidentally given, will prove not unacceptable to such instructors as may not have at hand the original works, and that this new piece of apparatus may contribute somewhat toward imparting a clearer understanding of the phenomena of waves.

The apparatus has been patented, and is manufactured by Messrs. E. S. Ritchie and Son, the well-known philosophical-instrument makers, of Boston, Massachusetts.

IV. *On the Velocity of Sound in Tubes.*

By A. KUNDT*.

TWO years ago the author laid before the Academy a method of determining the velocity of sound in elastic fluids, which consists in setting in vibration the column of air enclosed in a longitudinally vibrating tube. These vibrations may be made visible by light powder strewed in the tube; and by measuring the resultant dust-figures, the wave-lengths of the same exciting tone in different gases, and therewith the corresponding velocities of sound, may be readily found†.

Since it is not always certain in these experiments that the exciting tone of the longitudinally sounding glass tube remains in successive experiments exactly the same, the apparatus was first of all so altered that not only did one end of the longitudinally vibrating tube produce dust-figures in an enveloping tube, but also the other end in a second tube. If then the tube on one side was filled with the gas to be investigated, and that on the other with air, and the dust-figures were produced, the wave-lengths measured in the gas and in air, belonging to precisely the same tone, gave the exact ratio of the velocities of sound in the gas and in air.

When, however, the velocities of sound in the simple gases were examined by this method, it was observed that, in spite of the utmost care in purifying and drying the gases, and in spite of frequent endeavours, discrepancies were met with in the determinations, which seemed to indicate that sources of error as yet unknown influenced the result.

It was, moreover, ultimately found that these sources of error are not specially incident to the method used, but under certain circumstances always occur when sound is transmitted through tubes. The author accordingly subjected to a minute examination all the circumstances which might influence the velocities of sound in elastic fluids in tubes. At first, for the sake of simplicity, only air was used.

The mode in which the experiments were made was as follows:—The tube on one side of the apparatus was always unchanged, so that the dust-figures were always produced in it under the same conditions, and their length could therefore be taken as the normal length; on the other side were placed the tubes to be investigated, in which the velocity of sound was to be determined under varied conditions; and the dust-figures were simultaneously produced in the two. The ratio of the wave-

* Translated from the Berlin *Monatsbericht* for December 1867.

† Phil. Mag. January 1868, p. 41.

lengths of the two sides gives directly the ratio of the velocities of sound in the air in the two tubes.

The results of the investigation are as follows:—

(1) The velocity of sound in air is diminished when the diameter of the tube is decreased. The diminution, however, only becomes perceptible below a certain diameter; in all wide tubes the velocity of sound is equal. A decrease in the velocity is sometimes perceptible when the diameter of the tube is equal to a quarter wave-length of the tone used. The amount of the diminution of velocity with a decreasing diameter may be seen from the Table under (2).

(2) The diminution of the velocity of sound in air in tubes increases with the wave-length of the tone used. The following Table gives the velocities of sound of the air in five tubes of different diameters with three different notes, whose semiwave-lengths are as 90, 45, and 30 millims. The velocity of sound in the widest tube is equal to that in the open air = 332·8 metres.

Velocity of Sound for various Notes in glass tubes of varying diameter.

Semiwave-length of the note . . }	$\frac{L}{2} = 90$ millims.	$\frac{L}{2} = 45$ millims.	$\frac{L}{2} = 30$ millims.
	Velocity of sound, that in the widest tube being = 332·8 metres.		
Diameter of the tube.			
millims.	metres.	metres.	metres.
55·0	332·80	332·80	332·80
26·0	332·73	332·66	333·45
13·0	329·47	329·88	330·87
6·5	323·00	327·14	328·14
3·5	305·42	318·88	

(3) Powder strewed in a tube leaves the velocity of sound unaltered in wide tubes; in narrow ones it diminishes it the more the greater its quantity. This diminution may amount to 10 metres*.

(4) The influence of the powder is increased when it is very finely divided and is violently moved by the motion of the sound—as, for instance, when silicic acid is used.

(5) If the wall of the tube is roughened on the inside, or if a larger or a rougher surface is exposed to the air by means of paper or metal sheets inserted and fitting as close as possible the wall of the tube, the velocity of sound in narrow tubes is thereby materially diminished; and this diminution is considerably more than could be produced by the mere diminution in the section

* In the above experiments the quantity of powder used was so small that it could not appreciably influence the results.

of the tube by the interposed septum. In very wide tubes an alteration in the velocity of sound when the wall of the tube is altered is everywhere not perceptible.

(6) The velocity of sound in air is not appreciably dependent on the intensity of the note.

It follows from these results, that in all those cases in which it cannot be certainly established that the tubes are so wide as not at all to influence the velocity of sound, the value obtained is too small. In all the ordinary musical instruments, the diameter of the sounding columns of air, as compared with the wave-length, is small; and there must therefore be a diminution of the velocity of sound in all. The attempts to make the observed tones of stopped and of open pipes accord with theory are fruitless, if the velocity of sound in air is taken as a basis and not that in the pipe, which must always be first specially ascertained.

It has been already observed by Dulong that, apart from all corrections for the ends of tubes, the velocity was found to be too small, though this was not ascribed to the influence of the wall of the tube.

Two modes of explaining the changes observed in the velocity present themselves:—

Friction of the air in the tube might first of all diminish the velocity of sound. But it has already been proved elsewhere that the internal friction of air cannot diminish the velocity of sound in the open air; and from several reasons it is probable that in a tube (in which, certainly, the density of air increases towards the sides) the amplitude, but not the velocity, may be altered by friction. It is true that the friction in a tube has not been demonstrated to be without influence on the velocity.

Yet the origin of the discrepancies observed may probably be sought in an exchange of heat which takes place between the air which propagates the sound and the wall of the enveloping tube. Since part of the heat produced in the motion of sound is given to the wall, and an equal part of that consumed at another instant is replaced by the wall, the produced and consumed heat are not entirely used, as Laplace assumes, for the acceleration of sound. Hence the velocity of sound must be smaller. The factor which Laplace introduces into the formula no longer corresponds to $\frac{c}{c_1} = 1.41$, but, inasmuch as heat is exchanged, is smaller. If all the produced and consumed heat were imparted and restored, we should have $k=l$; that is, the velocity would sink from 332.8 to 280 metres, Newton's value. A diminution of one-half is in fact evident from the Table.

Since all the circumstances which increase the surface of the sounding column of air, as compared with its volume, favour an

exchange of heat between air and wall, it is clear that the velocity of sound, if an exchange of heat everywhere takes place, must be the smaller the narrower the tube, the rougher its wall, or the larger and rougher the surface of a second tube inserted. In like manner powder must have greater influence the finer it is and the narrower the tube; for it is then better distributed in the sounding air*.

The greater, finally, the duration of the oscillation of a tone, or its wave-length, the more considerable is the exchange in the longer time; hence the diminution of velocity must, under the same circumstances, be greater.

Since, as has been said, all the influences of the wall, of the powder, and so forth are infinitely small in their action on the velocities when the diameter of the tube reaches a certain magnitude, the method, notwithstanding the discrepancies observed, may be used for exact determinations of wave-lengths, and therefore of velocities of sound, if the tubes for the dust-waves are wide enough. The author had proposed, in the next place, to test more extensively than was hitherto possible two deductions from the theory—that is, that the velocity of sound under different pressures is the same, and that its variation by the temperature is expressed by the factor $\sqrt{1 + \alpha t}$.

The velocity of sound in air was determined in wide tubes between pressures of 400 and 1760 millims. Within these limits of pressure, which correspond to a difference in pressure of nearly two atmospheres, the velocity of sound remained quite constant.

To ascertain whether the influence of the diameter of the tubes, when narrower ones are used, is the same under different pressures, the velocity was determined in some narrower tubes under a pressure of from $\frac{1}{2}$ to $2\frac{1}{2}$ atmospheres. Using a note whose semiwave-length was 45 millims. in a tube of 3.5 millims. diameter, the velocity of sound under a pressure of $2\frac{1}{2}$ atmospheres was about 4 metres greater than under a pressure of half an atmosphere.

The author refers this to the circumstance that, with air at greater density, the greater quantity of heat produced and consumed is not exchanged with the wall in the same ratio as the smaller quantity of heat with a smaller density.

The velocity of sound at 0° and 100° was then determined, and in tubes which were so wide that their walls had no influence. One side of the apparatus with one dust-wave tube being immersed in melting ice, the other with its dust-wave tube was

* Powder, by its increasing the mass to be moved, might somewhat impede the velocity. I have convinced myself, however, by experiments which will be discussed in the complete paper, that this circumstance is inadequate for the explanation of the considerable variations.

heated in steam, and the wave-lengths of the same tone determined at 0° and 100° , which thus gave the ratio of the velocities of sound.

The following Table gives, as the result of seven experiments, the velocity of sound for 100° , taking the velocity at 0° as 332·8 metres :—

	Metres.
1.	388·20
2.	388·84
3.	389·15
4.	388·47
5.	389·02
6.	389·64
7.	388·60
Mean . . .	<u>388·99</u>

Calculating the value from the formula $332\cdot8\sqrt{1+100\alpha}$ ($\alpha=0\cdot003665$), we get 389·03. The difference of the observed and of the calculated value amounts therefore to 0·04 metre.

It is therefore proved that up to 100° C. the change in the velocity of sound is accurately expressed by the factor $\sqrt{1+\alpha t}$, and therefore that Laplace's factor $\frac{c}{c_1}$, the ratio of the specific heats of air, is constant within very narrow limits.

As this constancy is thus experimentally proved, observations of the velocity of sound at 100° may conversely be used to determine α , the coefficient of expansion of air.

Although α is not obtained with excessive accuracy from a single determination, the deviations of α in various observations are not too far apart; and from the mean of the seven observations we get

$$\alpha=0\cdot003662.$$

We see from this that, in spite of the considerable changes in the velocity of sound in tubes, the method in question with a suitable choice of apparatus yields very accurate determinations; and, in the future, accurate values for the velocities of sound of different gases, and therewith of the ratios of their specific heats, may be expected from it.

V. *On the Theory of Pressure in Fluids.* By R. MOON, M.A.,
Honorary Fellow of Queen's College, Cambridge*.

I DESIRE to commend the following points in the theory of pressure in fluids to the attention of mathematicians.

I. Suppose that we have a closed cylinder filled with air, and, for the sake of precision of statement, suppose the axis of the cylinder to be horizontal. Suppose also that at the time t every particle of the air is destitute of velocity, while the density of the air varies on this wise; viz., the air to the left hand of a vertical plane which bisects the axis of the cylinder has the uniform density D , while the air to the right of the same plane has the density $2D$.

Under these circumstances, according to the received theory with regard to the law of pressure in fluids, the air in the right-hand half of the cylinder will exert on the air in the left-hand half exactly double the pressure which the air in the left-hand half exerts on the air in the right-hand half; that is, the law of pressure of the received theory contradicts, in this case, the universal principle that action and reaction are equal and opposite.

II. Suppose that we have a vertical cylinder closed at its lower end, and having an air-tight piston capable of moving freely in the upper part of it. Below the piston the cylinder is filled with air, which is kept in equilibrium by means of a weight, W , resting on the piston, above which there is a vacuum.

If a second weight, W , be placed upon the piston, we know that the equilibrium will be destroyed. Let us see, however, how, according to the received theory of pressure, a thin stratum of air immediately beneath the piston will comport itself under these circumstances.

The pressure of the piston upon this lamina and the pressure of the lamina upon the piston must under *all* circumstances be equal and opposite.

Consequently, if the principle of the received theory be true (viz. that the pressure varies only with the density), the pressure of the air upon the piston (and therefore the pressure of the piston upon the air) must remain unaltered until a change has taken place in the density of the air in the lamina.

Now to a change in the density of the air in the lamina one thing is essential, viz. relative motion of the particles of which the lamina is composed; and to such motion of the particles two things are requisite, viz. :—

(1) A preexisting variation of pressure in the lamina, through the operation of which relative motion of its particles may be generated.

* Communicated by the Author.

(2) An interval of time during which such variation of pressure may act.

But while it has thus been seen that variation of density in the lamina must be preceded by variation of pressure in and upon the lamina, the received theory asserts that these two things, viz. variation of density and variation of pressure, are necessarily coexistent.

Hence in the case just stated, since at the time t , when the second weight is introduced, there is no variation in the density of the air in the lamina immediately beneath the piston, it follows, according to the received theory, that when the second weight is introduced there will be no variation of pressure in the lamina, and therefore nothing to generate that which, according to the received theory, is the essential mark and test of a change of pressure, viz. variation of density. According to the received theory, therefore, the equilibrium will be maintained after the second weight has been introduced, which is absurd.

It is clear that in the case just considered, the effect of the introduction of the second weight will be to propagate instantaneously into the air beneath the piston a pressure of finite amount, extending to a finite distance below the piston—such additional pressure having its maximum immediately below the piston, and thence diminishing gradually till, if the portion of the cylinder below the piston be long enough, it will finally vanish. If the portion of the cylinder below the piston be small, as compared with the magnitude of the weight resting upon the piston and with the density of the air, such additional pressure will extend throughout the entire mass of air beneath the piston; and we shall have, besides, an additional pressure of finite amount exerted simultaneously upon the base of the cylinder.

III. Suppose that we have a vertical cylinder having its lower end closed and resting upon a solid pier, and having, as before, an air-tight piston capable of moving freely in the upper part of it. Above the piston is a vacuum, while below it the cylinder is filled with air which at the time t is destitute of velocity, but of which the density varies according to the following law, viz.

$$\rho = D\varepsilon^{ax},$$

where x denotes the vertical distance from the base of the tube. Gravity is supposed not to act; but a constant downward pressure is exerted upon the piston equal in amount to the pressure which, according to the received theory, would be due to the air immediately beneath the piston at the time t .

Under these circumstances the effective force which, according to the received theory as to the pressure in fluids, will be exerted upon each particle of the air at the time t will be uniform. As-

sume this force to be f , then at the end of the time $t + dt$ each particle of the air will have acquired a velocity $f dt$, and will have described a space $\frac{f dt^2}{2}$.

Moreover, since the motion during the time dt (being such as has been described) can in no way have altered the density, it is clear that at the end of the second interval dt each particle will have acquired a velocity $2f dt$, and will have described a space $2f dt^2$, the density remaining unaltered.

Reasoning from step to step in the same manner, at the end of the finite time t_1 , each particle will have acquired a velocity ft_1 , and will have described the space $\frac{ft_1^2}{2}$.

It is obvious, however, that no such effects as those above described, and which, if the received theory of pressure be universally true, must necessarily take place, can occur in the case we are considering. At the end of the time $t + t_1$, equally as at the end of the time $t + dt$, the particles in contact with the base of the cylinder will have described *no* space, they can have acquired *no* velocity.

It is clear, in fact, that any theory of pressure which, under the above circumstances, would render the value of $\frac{dp}{dx}$ other than zero, as the received theory on the subject does render it, must be erroneous.

IV. Suppose that we have a vertical cylinder, closed at both ends and resting on a solid pier, filled with air which is in equilibrium, gravity being supposed to act.

Suppose that at the time t a second force begins to act, which is equal to the force of gravity and acts in the same direction.

Under these circumstances, either the law of pressure in the air in the cylinder will be instantaneously altered or it will not.

If it be instantaneously altered, the received theory as to the law of pressure is contradicted; for it is impossible that the density of the air in the cylinder can be altered instantaneously, change of density implying motion, for which lapse of time is indispensable.

But if the pressure throughout the air be not instantaneously altered, the following consequence will occur; viz. each particle of the air will be acted upon (1) by the forces which acted upon it before the time t , which forces destroy each other, since before t the air was in equilibrium; (2) by the constant force g . Hence at the end of the time $t + dt$ each particle will have acquired the velocity $g dt$, and will have described the space $\frac{g dt^2}{2}$, no change taking place in the density of the air.

And, reasoning as in the last case, at the end of the time $t + t_1$ each particle will have acquired the velocity gt_1 , and will have described the space $\frac{gt_1^2}{2}$, results which, it is obvious, are not true as regards the particles at the base of the cylinder.

In this case also, therefore, it is evident that the adoption of the received theory of pressure has led us to untrue results*.

6 New Square, Lincoln's Inn,
June 10, 1868.

VI. *Description of a new Thermostat.* By FREDERICK GUTHRIE, Ph.D., F.R.S.E., Professor of Physics and Chemistry, Royal College, Mauritius†.

IN physical and chemical research, and also in the arts, the problem of maintaining a constant temperature frequently presents itself. To this end Bunsen constructed his ingenious thermostat, whose action depends upon the expansion of mercury. For many purposes Bunsen's thermostat answers admirably; but in cases where a perfectly constant temperature is required, I have not found it sufficiently nimble in its action of adjustment.

Requiring, for some experiments on heat, to maintain water for many hours at a temperature approaching to absolute constancy, I constructed the thermostat here figured, and have found it to answer very satisfactorily.

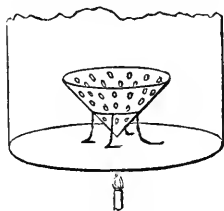
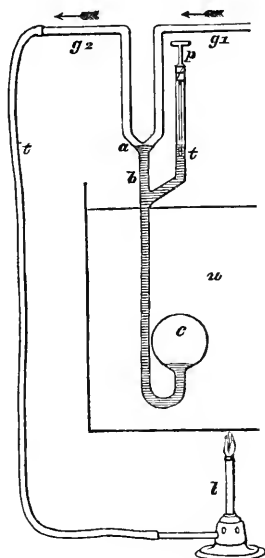
As others may have experienced the same want, and as I am not able to find any description of a similar instrument, I beg to submit the following description to the readers of the *Philosophical Magazine*.

I may state at once that, with ordinary care and by using the "distributor" afterwards described, I have no difficulty in keeping a temperature constant to $0^{\circ} \cdot 2$ Fahrenheit for six hours, including the time when, towards evening, an additional pressure is applied through the service-pipe. The gas from the service-pipe enters at g_1 , passes through g_2 , and is conducted by the flexible tube t to the lamp which heats the reservoir of water, u . The tubes g_1 and g_2 join to form the tube b . The lower extre-

* In the above case we must have $\frac{dp}{dx} = 0$ at the base of the cylinder at the time t when the second force begins to act, the value of $\frac{dp}{dx}$ taken without regard to sign increasing as we ascend. The action of the second force will also cause an instantaneous increase of finite amount in the pressure on the base of the cylinder.

† Communicated by the Author.

mity of b passes into the water and carries a bulb, c . The tube b also carries a branch, t , the upper end of which carries a cork through which works very stiffly the solid plug p . The cork in t is provided with a small hole to allow air to pass. Mercury is poured into t until it rises to a convenient height in b . The plug p is adjusted and the whole held very firmly in a clamp. The action is obvious. At a certain temperature the air in c will occupy such a volume that the mercury will be forced up b until its surface, a , begins to shut off the gas. The flame of l will thereupon diminish in size. By pushing the plug p further down mercury will be forced up b , and a consequent lowering of the temperature will follow. By this means the instrument can be set for any temperature without disturbing the mercury.



To avoid the necessity of constant stirring, and to disperse the currents of heated water rising from below, a perforate cone of sheet iron, as shown in the accompanying figure, is supported upon the bottom of the vessel containing the water*.

VII. On the Expansion of Solids by Heat.

By M. H. FIZEAU†.

IN the new research which I have the honour of laying before the Academy, I propose to present the continuation of my researches on the expansion of solids by heat, and especially of crystallized bodies.

Having observed a great number of new bodies, and having investigated them in various directions with a view to make evident

* Since the above was in type, I have been informed that Bunsen also has employed the expansion of air in his thermostat. There remains, therefore, so little originality in my form of the apparatus, that the inconvenience which would ensue to the printers of the Magazine alone prevents the suppression of the above.—F. G.

† Translated from the *Comptes Rendus*, May 25, 1868.

a general law which appears to rule these phenomena, having, moreover, lately succeeded, thanks to the able advice of M. Des Cloiseaux, in observing with certainty some more complex crystals belonging to the oblique system, I may now attempt to develop with more detail, and with the sanction of experiment, the theoretical views laid down in my first memoir*.

Some remarks may first be made and some principles laid down with the view of removing objections which might be raised relative to the regularity and constancy of the phenomena in question.

All observations agree in showing that the phenomenon of the change of volume of a body by heat, whether it be an expansion or a contraction, is produced in a continuous manner and with a regularity perfectly similar to that of the changes in temperature—the same volumes corresponding constantly and identically to the same temperatures.

Any idea of sudden and accidental, and, so to say, capricious variations may then be rejected in this class of phenomena, as well as the existence of slow variations which in the course of time might occur in the numerical value of the coefficients of expansion.

I may cite, in support of the invariability of these coefficients, two determinations made with great care, at an interval of a year, on the same quartz crystal supported on the same platinum tripod,—this latter having during this interval undergone several hundred alternations of temperature between 7° and 80° . These two coefficients correspond to the direction of the axis of the crystal.

First determination . $\alpha = 0.0000078118$,

Second determination $\alpha = 0.0000078117$.

When, in my former memoir, I endeavoured to coordinate the phenomena then known regarding the expansion of crystals belonging to the various crystalline systems, the limited number of the observations might indeed indicate a simple and general law, but did not permit its being established with sufficient precision; yet, guided by the analogies which were already manifest between the principal characters of these phenomena and those which the phenomena of luminous and calorific propagation present in their relations to the general symmetry of crystals, I have entered upon the path which for some time has been opened to us by Fresnel's celebrated researches on the propagation of light in crystals—a path which has been successfully followed by Senarmont in his discoveries of the unequal propagation of heat in the same bodies. And just as these two classes

* May 21 and 28, 1866.

of facts have been connected by very general geometrical considerations, in like manner for the phenomena of expansion we may, by analogous considerations, express the law of the variations met with in the numerical values of the expansions when they are considered according to different directions. There are then three distinct physical phenomena which may be connected with analogous theoretical views; these are the propagation of light and of heat through crystals, and the expansion by heat of the crystal itself; and these theoretical views are precisely of the nature of those which geometricians use when they investigate ellipsoidal surfaces. In fact, one and the same principle serves as a common starting-point in the theoretical explanation of these three orders of phenomena—that is, the consideration of the three principal directions or rectangular axes endowed with well-defined physical and geometrical properties, and around which are attached as rigorous consequences the totality of these phenomena in their most varied manifestations.

I must here simply attempt to define the action of these axes relatively to the phenomena of expansion with which we are occupied; and I shall adduce a certain number of experiments which clearly prove that these axes correspond to real and distinct physical properties, which prevent us from regarding them as a mere geometric fiction sufficient to group empirically the data of observation. For the future they will be designated as *axes of expansion*. The expression *axes of elasticity* used in my first paper having become inadequate owing to the results observed in oblique crystals, we shall see in the sequel that in these crystals the three kinds of axes are no longer superposed as in other crystalline systems, but are really separated from each other by angular distances which are frequently considerable.

It has been shown in the first memoir that, if it be attempted to express in a general manner the value of the expansion of a crystal in any direction referred to three rectangular axes, a very simple formula is obtained, merely containing the squares of the cosines of the angles made with the three axes, as well as the three principal coefficients of expansion corresponding to these axes.

But it is important to remark that the reasoning which has led to this result depends really on the following principle:—

However complex be the crystalline form, however varied the expansions observed in the various directions, be they even in one case expansions, in another contractions, there are only three primitive expansions distinct and independent of each other, and solely manifested in three fixed directions at right angles to each

other ; these are the three axes of expansion. In any other direction than those, there will be observed only the simultaneous effects of these three primitive expansions, which will always manifest themselves individually, according to their intensities and their own directions, and constant for the unit of length.

It may be added that, all the elementary parts of the crystal being identical among themselves, these axes are not represented by three lines having a determinate position in the interior of the crystal, but really by three rectangular systems of parallel lines taken at each of the internal points.

To complete the enunciation of what appears essential in these singular properties, which in some sense seem to indicate a tri-nary arrangement in the elements of the crystallized substance, it remains to be stated in what each of the principal expansions must differ from any other resultant expansion, or, in other words, what is the distinctive character of an axis of expansion.

Suppose a sphere isolated in the body of the crystal at a certain temperature ; if it be heated, the sphere will expand unequally in the direction of its different radii, and in the most general case its form will be ellipsoidal ; but there will always be three diameters rectangular to each other, at the ends of which the displacement of a point on the surface of the sphere will be in a radial direction—that is to say, in the prolongation of the radius itself and without any lateral deviation.

That is the true character of axes of expansion, and the principle of the geometrical construction from which, in the first memoir, we have deduced the formula which we here examine.

I shall now give the results of experiments made with a view of controlling the accuracy of the general formula in many of its most important consequences, by trying to find in the various crystalline systems the most decisive phenomena and those most accessible to observation.

Denoting by D the coefficient of expansion in any direction whatever, determined by the angles δ , δ' , and δ'' which this direction makes with the three axes of expansion, and calling α , α' , and α'' the three coefficients of expansion along the three axes, we have the following relation,

$$D = \alpha \cos^2 \delta + \alpha' \cos^2 \delta' + \alpha'' \cos^2 \delta'' ; \quad . \quad . \quad . \quad (1)$$

but we have at the same time the known relation which expresses that the three angles δ , δ' , and δ'' are referred to three rectangular axes,

$$\cos^2 \delta + \cos^2 \delta' + \cos^2 \delta'' = 1. \quad . \quad . \quad . \quad (2)$$

Cubical System.

The general characters and the properties of this crystalline system lead us to consider the three principal expansions equal

to each other ; that is to say,

$$\alpha = \alpha' = \alpha'' ;$$

equation (1) then becomes

$$D = \alpha (\cos^2 \delta + \cos^2 \delta' + \cos^2 \delta''),$$

and, by equation (2),

$$D = \alpha ;$$

that is to say, the expansion is constant, independent of the direction in question, and always equal to that which takes place along the axes, the position of which cannot be revealed by any difference in the expansions, and which must be regarded as indeterminate. The following are some observations which refer to crystals of this system. These numbers and the following represent the linear expansion for the unit of length for 1 degree at the point $\theta = 40^\circ$ of the thermometric scale.

Fluor-spar.—Normal to a face of octahedral cleavage,

$$\alpha = 0.00001911,$$

$$\alpha = 0.00001910 ;$$

on a face of the cube (another crystal),

$$\alpha = 0.00001910 ;$$

on a face cut at an angle of 5° to a face of the cube (another crystal),

$$\alpha = 0.00001915.$$

Galena.—At right angles to a face of eubic cleavage,

$$\alpha = 0.00002014 ;$$

on a cut octahedral face,

$$\alpha = 0.00002014.$$

Cubic pyrites.—At right angles to a natural face of the cube,

$$\alpha = 0.00000907 ;$$

on a face cut in a group of Peruvian crystals without any common direction,

$$\alpha = 0.00000908.$$

Suboxide of Copper.—At right angles to a face of the rhomboidal dodecahedron,

$$\alpha = 0.00000093 ;$$

on a face situated at 90° from the foregoing,

$$\alpha = 0.00000093 ;$$

on a face cut in a group of crystals without any common direction,

$$\alpha = 0.00000093.$$

Dimetric and Hexagonal or Rhombohedral Systems.

These two systems, distinct in crystallographic relations, merge into each other in their optical phenomena, their conductivity for

heat, and their mode of expansion, which appears to depend on their symmetrical structure about a principal crystallographical axis. The expansions are here different in different directions; and reasons deduced from the symmetry of the structure show that one of the axes of expansion must coincide with the principal crystallographical axis, the two others being perpendicular to it. If we assume, by the same reasons of symmetry, equality in the coefficients corresponding to these two latter axes of expansion, where $\alpha' = \alpha''$, equation (1) will become

$$D = \alpha \cos^2 \delta + \alpha' (\cos^2 \delta' + \cos^2 \delta'');$$

but equation (2) gives

$$\cos^2 \delta' + \cos^2 \delta'' = 1 - \cos^2 \delta = \sin^2 \delta;$$

we shall then have (α being the expansion for the former axis)

$$D = \alpha \cos^2 \delta + \alpha' \sin^2 \delta. \quad . \quad . \quad . \quad . \quad (3)$$

Taking any direction at right angles to the first axis, which gives

$$\delta = 90^\circ, \quad \cos^2 \delta = 0, \quad \sin^2 \delta = 1,$$

the equation is reduced to

$$D = \alpha';$$

that is to say that, in any direction at right angles to the first axis, which coincides with the axis of symmetry, the expansion is constant, and that it is impossible to distinguish the axes of expansion.

In any direction making with the first axis the same angle $\delta = 54^\circ 44'$, we have

$$\cos^2 \delta = \frac{1}{3} \quad \text{and} \quad \sin^2 \delta = \frac{2}{3},$$

and equation (3) becomes

$$D = \frac{\alpha + 2\alpha'}{3},$$

which is precisely the expression for the mean linear expansion of the crystal.

Varied experiments have been made on various kinds of crystals with a view of verifying the accuracy of the following remarkable property, which is deduced from theory:—All crystals which have the form of right prisms, of rhombohedra, of regular hexagons, or forms derived from these, present a certain angular direction (the same for all crystals) which makes with the principal crystallographical axis an angle of $54^\circ 44'$ (an angle for which $\cos^2 \delta = \frac{1}{3}$); and along this direction we ought to find exactly the third of the cubical expansion, or the mean linear expansion for each crystal.

It has been seen before that in the most general case this condition must be satisfied by the normals to the faces of a regular

octahedron suitably arranged; but in the present case this octahedron of mean expansion must be conceived as having one axis coinciding with the principal axis of the crystal, while the two others may be arranged in any manner whatever.

To make this decisive comparison between experiment and the result deduced from theory, three expansions must evidently be measured, α , α' , α^m , in the three following directions: the first parallel, and the second perpendicular to the principal crystallographical axis, the third making an angle of $54^\circ 44'$ with this axis; while the two latter directions may be in any azimuth whatever around the same axis.

Under these circumstances the cubical expansion will be

$$\alpha^{\text{cub}} = \alpha + 2\alpha';$$

the mean linear expansion,

$$\alpha^{\text{lin}} = \frac{\alpha + 2\alpha'}{3};$$

and the theory will only be satisfied provided this latter value coincide with the value found directly for α^m —that is, if we have

$$\alpha^m = \alpha^{\text{lin}}.$$

The following are the results of these observations:—

α is the expansion along the principal crystallographic axis.

α' along the normal to this same axis.

α^{lin} the mean linear expansion deduced from α and from α' .

α^m the expansion in the direction of the angle $54^\circ 44'$, with a possible error of $5'$ to $10'$ in the cutting of the crystal*.

Siberian Zircon. (Prism with square base.)

α	=	0.00000443
α'	=	0.00000233
α^{lin}	=	0.00000303
α^m	=	0.00000304

Emerald (Beryl). (Hexagonal.)

α	=	-0.00000106
α'	=	0.00000137
α^{lin}	=	0.00000056
α^m	=	0.00000057

Iceland Spar. (Rhombohedral.)

α	=	0.00002621
α'	=	-0.00000540
α^{lin}	=	0.00000514
α^m	=	0.00000507

* I must here render fresh testimony to the remarkable skill and the intelligent zeal with which M. Henri Soleil has cut the crystals used in these researches.

Quartz. (Rhombohedral.)

α	=0.00000781
α'	=0.00001419
α^{lin}	=0.00001206
α^{m}	=0.00001206

Bismuth. (Rhombohedral.)

α	=0.00001621
α'	=0.00001208

The third observation was made at right angles to a natural face of the rhombohedron—that is, at an angle of $56^{\circ} 24'$ with the principal axis. The corresponding calculation was made by means of formula (3) :

Calculation	.	.	.	α	=0.00001334
Observation	.	.	.	α	=0.00001338

Rhombic System.

As the situation of the axes of expansion is always subordinate to the crystalline structure, it must be admitted that to identical directions, as regards symmetry of faces, there correspond identical expansions—a principle which leads directly to fixing the situation of the three axes of expansion in this system parallel to the three crystallographical axes.

Suppose, in fact, a rectangular parallelepipedon constructed on the three crystallographic axes as edges; it will be at once seen that during the expansion of this solid the displacement of any point upon one of its faces can only take place parallel to the edge at right angles to this face and without lateral deviation, which is the character of the axial expansion; for if it were otherwise, and if the displacement took place along a certain inclined direction, it would be sufficient to turn the crystal through 180° in the plane of the face in question to find another direction in which the crystallographical relations would be the same, and along which, consequently, the same inclination would be produced.

The same kind of considerations may be employed to determine the position of the first axis in the dimetric prism previously considered, as well as in the hexagonal prism. For the case of the rhombohedron an analogous conclusion is reached by attributing to the crystal two successive rotations of 120° .

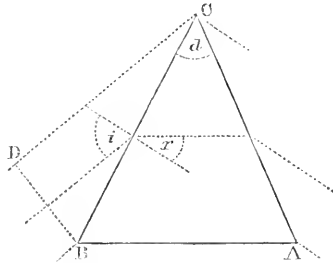
A subsequent communication will be devoted to completing the subject of the right rhomboidal system, and to considering the oblique rhomboidal system.

VIII. *On the comparative Efficiency of different Forms of the Spectroscope.* By EDWARD C. PICKERING*.

IT is the object of the present paper to furnish a means of comparing with accuracy spectroscope prisms of different forms, and to determine what must be their refracting angle to produce the greatest dispersion with the least loss of light. We have then to consider the dispersion, the loss by reflection, and that by absorption.

1. *Dispersion.*—The dispersion of any part of the spectrum is proportional to the angular interval between two rays of nearly equal refrangibility as the two parts of a double line.

Let α be the refracting angle of the prism ABC, n the index of refraction of the less refrangible ray. For minimum deviation,



$$r = \frac{\alpha}{2}, \sin i = n \sin \frac{\alpha}{2} \dots \dots \dots (1)$$

If dn be the difference of the indices of refraction of the two rays, di will be their angular divergence. Differentiating (1),

$$di = \frac{\sin \frac{\alpha}{2}}{\cos i} dn = \frac{1}{n} \tan i dn, \dots \dots \dots (2)$$

in which $\frac{1}{n} \tan i$ serves as a measure of the dispersion under different angles of incidence. It differs essentially (when the angle of incidence is large), from the deviation, which is commonly, but incorrectly, assumed as the measure.

Comparative Dispersion and Deviation of a Ray entering a medium in which $n=1.5$.

Angle of incidence $i \dots \dots$	0°	15°	30°	45°	56° 19'	60°	75°	80°	85°	90°
Dispersion $\frac{1}{n} \tan i \dots$	0	·179	·385	·667	1·000	1·1565	2·488	3·781	7·620	∞
Deviation $i-r \dots$	0°	5° 4'	10° 32'	16° 53'	22° 38'	24° 44'	34° 55'	38° 58'	43° 23'	48° 11'
Deviation reduced to same unit as dispersion.....	0	·231	·465	·746	1·000	1·093	1·543	1·720	1·917	2·121

* From the American Journal of Science and Arts, vol. xlv. (May 1868). Communicated by the Author.

The dispersion then increases much more rapidly than the deviation; hence in spectroscopes whose deviation is the same, that one will disperse most, in which i and therefore α is the greatest.

The above discussion applies strictly only to the emergent ray; but in the position of minimum deviation, the dispersion of a prism is just double this, as may be seen from the general formula for dispersion (Radicke's 'Optics,' vol. i. p. 179). I propose hereafter to discuss the question whether greater dispersion with the same loss of light could not be obtained by some other position of the prism.

2. *Reflection*.—In estimating the loss by reflection, it is usual to assume that the same proportion of the incident light is lost at each successive refraction. But in reality the light so refracted is partially polarized, and in this state another law determines the amount reflected. Fresnel showed that of a ray polarized in the plane of incidence, the proportion reflected

$$B' = \frac{\tan^2(i-r)}{\tan^2(i+r)},$$

while a ray polarized in a plane perpendicular to the first, would lose by reflection

$$A' = \frac{\sin^2(i-r)}{\sin^2(i+r)}.$$

Regarding common light as composed of two beams of equal intensity polarized at right angles, the amount reflected would be $\frac{1}{2}A' + \frac{1}{2}B'$, and that transmitted $\frac{1}{2}[(1-A') + (1-B')]$. On meeting a second surface inclined at the same angle of incidence, the amount transmitted would be $\frac{1}{2}[(1-A')^2 + (1-B')^2]$, and after passing m surfaces $\frac{1}{2}[(1-A')^m + (1-B')^m]$.

This formula can be applied directly to the m surfaces of the prisms of a spectroscope, since in the position of minimum deviation i and r are the same for all, and therefore the amount transmitted is the same, whether the passage is from glass to air or from air to glass.

The formulæ of Fresnel are used in preference to those of Cauchy, although the latter have been proved by Jamin and others to be more correct. But the coefficient of ellipticity which they involve is neither so commonly nor so easily found as the index of refraction. Furthermore, for glass the difference would probably be so small that it could be neglected.

3. *Absorption*.—The average length of glass traversed by the light is one-half the base AB, multiplied by N the number of prisms; and the amount escaping absorption is proportional to

the logarithm of this distance, or to $\log N \times BC \sin \frac{1}{2} \alpha$, or in prisms admitting the same amount of light (that is, in which BD is the same) to $\log BD \times N \frac{\sin \frac{1}{2} \alpha}{\cos i}$, since $BC = \frac{BD}{\cos i}$; but the dispersion is proportional to $N \frac{\sin \frac{1}{2} \alpha}{\cos i}$; hence in spectroscopes dispersing equally, and composed of prisms of the same material, *the loss by absorption will be the same in all*; so that, as far as the absorption is concerned, it makes no difference whether a spectroscope is composed of a large number of very acute-angled prisms, or of a less number in which the angle is more obtuse.

Thus we avoid the difficulty which seemed at first sight insurmountable, since the actual amount of light absorbed varies not only with the material, but with the refrangibility of the rays, and according to laws not yet discovered.

The following Tables give the deviation, dispersion, and amount of light escaping reflection, of spectroscopes composed of from one to ten prisms of indices of refraction 1.5, 1.6, and 1.7.

Table I. applies to prisms of 45° , Table II. to those of 60° (these being the forms in general use), and Table III. where the angle is such that the reflected light would be totally polarized (α being $67^\circ 22'$, 64° , and $60^\circ 56'$ in the three cases respectively). This form of prism appears to present great advantages for large spectroscopes, since at most but one-half of the light can be reflected, while one prism disperses as much as two of 45° .

TABLE I.— 45° Prisms.

	<i>n</i> .	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation	1.5	12° 32'	25° 4'	50° 8'	75° 12'	100° 16'	125° 20'	250° 40'
$i - \frac{\alpha}{2}$...	1.6	15° 15'	30° 30'	61° 0'	91° 30'	122° 0'	152° 30'	305° 0'
	1.7	18° 5'	36° 10'	72° 20'	108° 30'	144° 40'	180° 50'	361° 40'
Dispersion	1.5	.467	.935	1.870	2.804	3.739	4.674	9.348
$\sin \frac{\alpha}{2}$	1.6	.484	.968	1.936	2.904	3.872	4.840	9.680
$\frac{\sin \frac{\alpha}{2}}{\cos i}$...	1.7	.504	1.008	2.016	3.023	4.031	5.039	10.078
Transmitted	1.5	.957	.916	.841	.774	.724	.661	.461
$\frac{1}{2}[(1-A')^m$	1.6	.943	.892	.799	.719	.651	.592	.391
$+ (1-B')^m]$	1.7	.926	.859	.745	.653	.578	.516	.324

TABLE II.—60° Prisms.

	<i>n.</i>	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation ...	1.5	18° 35'	37° 10'	74° 20'	111° 30'	148° 40'	185° 50'	371° 40'
	1.6	23° 8'	46° 16'	92° 32'	138° 48'	185° 4'	231° 20'	462° 40'
	1.7	28° 13'	56° 26'	112° 52'	169° 24'	225° 44'	282° 10'	564° 20'
Dispersion ...	1.5	.756	1.512	3.023	4.535	6.046	7.558	15.116
	1.6	.833	1.667	3.334	5.000	6.667	8.334	16.668
	1.7	.949	1.899	3.797	5.696	7.594	9.493	18.986
Transmitted .	1.5	.945	.895	.811	.742	.686	.641	.509
	1.6	.920	.853	.748	.672	.618	.578	.491
	1.7	.888	.801	.681	.608	.565	.538	.505

TABLE III.—Angles of Prisms 67° 22', 64°, and 60° 56'.

	<i>n.</i>	1 sur- face.	1 prism.	2 prisms.	3 prisms.	4 prisms.	5 prisms.	10 prisms.
Deviation ...	1.5	22° 38'	45° 16'	90° 32'	135° 48'	181° 4'	226° 20'	452° 40'
	1.6	26°	52°	104°	156°	208°	260°	520°
	1.7	29° 4'	58° 8'	116° 16'	174° 24'	232° 32'	290° 40'	580° 20'
Dispersion ...	1.5							
	1.6	1	2	4	6	8	10	20
	1.7							
Transmitted .	1.5	.923	.863	.763	.691	.639	.600	.520
	1.6	.899	.818	.702	.629	.582	.552	.505
	1.7	.874	.780	.657	.588	.549	.523	.501

To apply these Tables to an example, let us compare three spectroscopes of ten prisms each, of angles 45°, 60°, and 64°, the index of refraction being 1.6.

10 prisms of	Deviation.	Dispersion.	Transmitted.	Trans. $\times \cos i$.
45°	305° 0'	9.680	.3911	.308
60	462 40	16.668	.4912	.291
64	520	20.000	.505	.268

Again, comparing spectroscopes producing equal deviation,

	Deviation.	Dispersion.	Transmitted.	Trans. $\times \cos i$.
12 prisms of 45°	366° 0'	11.616	.339	.268
8 " 60	370 8	13.334	.532	.319
7 " 64	364	14	.521	.276

From the first example we see that by using 64° prisms instead of 45° , we obtain more than double the dispersion, with even less loss of light, while in the second case seven 64° prisms prove much more efficient than twelve of 45° .

All these calculations seem to point to the superiority of 60° prisms over those of 45° . A much greater angle is objectionable from the increased distortion produced by the slightest imperfection in the refracting faces.

In prisms admitting the same amount of light, the more acute the angle the less is the quantity of glass and the less the area of each face. The ground might be taken that a 45° prism could be made larger than one of 60° at the same expense, and thus the difference in light remedied. In this case, however, it would be necessary to enlarge the telescope, number of prisms, and in fact the whole instrument. Even supposing this change made, the prisms of larger angle preserve their superiority, though not in so marked a degree. The calculation is readily made by multiplying the transmitted light by $\cos i$, as is done in the above examples.

The index of refraction varying with the refrangibility of the rays, the dispersion, loss of light, &c. would vary in different parts of the spectrum. The change would, however, be small, and could be determined, if necessary, by merely altering n .

Institute of Technology, Boston,
February 29, 1868.

IX. *An Optical Experiment.* By JOHN C. DOUGLAS, *East-India Government Telegraph Department*.*

THE following experiment (or rather experiments) furnishes a new means of demonstrating experimentally several laws of vision, particularly such as relate to visual direction; and, suitably modified, may possibly be of some practical value. Prepare a rather thick looking-glass by sprinkling its surface with prepared chalk or other fine powder, shaking off the superfluous powder so that the glass be covered with a thin coating of powder, through which the reflected image of the face may be seen with tolerable distinctness. If the glass so prepared be held 6 to 8 inches from the face of the observer, and so that he can see a reflection of his own features, on shutting one eye the powder will no longer appear equally distributed, but will appear arranged in lines radiating from a point on the glass in front of the reflected image of the pupil of the eye used in observing the phenomenon.

* Communicated by the Author.

If (the appearance described above being visible) the observer move his head with relation to the glass, the centre from which the lines appear to radiate and the whole system of lines move. If, however, the observer move his eyeball in its orbit, the glass and his head being stationary, the centre and system of radiating lines do not appear affected. On regarding the prepared glass with both eyes, he will see two centres, from which radiate two systems of lines, one centre being opposite to the image of the pupil of each eye. The appearance is not so distinctly seen, however, with two eyes as with one; and if one eye be stronger than the other, *i. e.* more effective for purposes of vision when both are open, the system of lines appertaining to the weaker eye is less distinct than that appertaining to the stronger. The superior distinctness of the appearance when one eye only is used is due to the fact that the eyes mutually interfere when used together; for it is not necessary to shut one eye, but merely to interpose a sheet of card or other opaque body between it and the mirror. I have never failed to distinguish the radiating appearance in my own case, nor in the many cases in which I have endeavoured to show it to unscientific persons; and I believe, if only one eye be used at first, and the instructions given above be strictly adhered to, the appearance may be readily seen by any person; but as I have heard of an instance in which the phenomenon could not be distinguished, I have thought it necessary to give rather minute instructions*.

The phenomenon is so easy of explanation that it is not necessary I should explain it, I will therefore confine myself to pointing out that the lines seen represent the apparent directions of the lines joining the particles of powder and their images, and it is evident that the centres of the systems of lines are in the perpendiculars to the surface of the mirror drawn through the centres of visual direction. The apparatus may be modified in accordance with the wants of each observer, the principle upon which its construction is based remaining the same; the glass may be of any attainable thickness; a glass plate may be placed in front of the mirror at various distances from it to carry the powder; engraved lines, dots, &c. may be employed in lieu of powder; figures or other characters may be written on the mirror or glass plate if required; and the whole instrument may be suitably mounted.

The applications of the instrument described appear numerous; a few are given below:—Experimental proof is afforded of the prin-

* The effect was most striking when the powder was very thinly and uniformly spread over a piece of plane glass placed before the mirror, at such a distance that the powder was rather less than half an inch from the reflecting surface.

cial laws of linear perspective—as the existence of vanishing points, that they are in a line drawn through the centre of visual direction parallel with the vanishing lines, that vision with one eye only can be considered in perspective, &c.; the coincidence of the centre of visual direction and the centre of motion of the eyeball is proved by the immobility of the radiating lines when the eyeball only is moved, and their movement when the head is moved with relation to the mirror; a means is afforded of finding a point on a looking-glass, a perpendicular to the surface drawn through which would pass through the centre of visual direction; a principle is afforded on which an instrument may be constructed for measuring the distance between the centres of motion of the eyes. The instrument I used for this purpose consisted of a card divided along one edge by lines drawn on both sides of it, and as nearly as possible coincident; this card being held in front of a mirror and viewed obliquely, so that the divisions on the card and on its image might be seen at the same time (the card being held with the divided edge horizontal), two lines on the card were seen to coincide with the images of two lines corresponding to them on the other side of the card; and the distance between the two lines so coinciding with reflected lines being read off on the card, gave the distance between the centres of motion of the eyeballs. This instrument admits of great improvement; and it will be evident that its delicacy may be increased by increasing the proportion the distance between the image and object bears to the distance of the mirror from the eyes. It has been stated that, if no apparent motion could be produced in a distant object by closing one eye after having covered that object by another nearer the eye, the eye so closed must be inoperative when both eyes are used, the greater perfection of the one eye obscuring altogether the effect of the weaker organ. And so far as apparent direction is concerned the experiment seems conclusive. But this view appears to need modification; for in a case in which both systems of lines were distinctly visible simultaneously, proving both eyes were employed, no apparent motion could be produced by closing the left eye; and although the left system of lines was fainter than the right, and the left eye was undoubtedly weaker than the right, the fact that the system appertaining to the left eye was distinctly visible when both eyes were used simultaneously proves that, in regarding the mirror, both eyes were effective.

It appears, therefore, that the powdered mirror affords a delicate means of detecting the share each eye has in vision, of exposing their inequalities of strength, and of detecting if both are actually effective when used together—in the last, being more delicate than the apparent-motion test.

Sir David Brewster (*Optics*, p. 294) has stated that the centre of visual direction coincides with the geometrical centre of the eyeball; but it has been objected to this, that Volkman's experiment on visual direction proves that the lines of visual direction cross each other in a point anterior to the geometrical centre, and that calculation will show this is inconsistent with the curvatures and refractive powers of the humours composing the refractive media of the eye; but as, in the powdered-mirror experiment, the grain of powder seen singly and forming the centre of the system of lines marks the point through which a perpendicular to the surface of the mirror represents the line of visual direction, and as this line is immoveable when the eyeball is moved, it follows that this line passes through a point in the eyeball stationary with regard to the eyeball, notwithstanding the motion of the eyeball; *i. e.* wherever the centre of visual direction may be, it is the centre of motion, if not the centre of figure, of the eyeball.

Madras, May 20, 1868.

X. *On Purification of Sewage.*

By G. W. WIGNER, *F.R.A.S.**

A NEW mode of purification of sewage by the addition of chemical agents has been introduced by Mr. Sillar and myself, and some experiments have been tried on a large scale at Tottenham. The process is a cheap one, and, as far as can be judged from the Tottenham experiments, is very successful. The time occupied in precipitation is very short, and the proportion of impurities removed is large.

A tank holding 5000 gallons was filled with sewage and clarified eight times in succession, the average time occupied in filling and settling being less than twenty minutes. The supernatant water was clear, and almost free from either taste or smell. The analyses below show that more than 80 per cent. of the ammonia originally present in the sewage was carried into the sediment. The produce of the 40,000 gallons was about 8 cwt. of manure, closely resembling guano in appearance. A large tank, holding 40,000 gallons, was afterwards filled and precipitated with equally satisfactory results.

Some more experiments are to take place shortly; and if these are successful, the details of the process will be published.

* Communicated by the Author.

Abstract of Analyses of Sewage &c.

Tottenham Experiments.

	Average sewage.	Average water from sewage.	Tottenham water.
Total solid matter per gallon . . . }	203·89	81·96	48·70
Organic matter . .	109·20	14·62	11·31
Ammonia	3·970	·584	
Phosphoric acid . .	7·23		
Common salt . . .	57·10	57·51	9·21
Silica, alumina, and various salts . . }	30·36	9·82	28·18

Sewage Residue or Manure.

Water	4·45
Organic matter containing ammonia	2·37.
Phosphoric acid	5·33
Sulphate of lime	1·67
Silica, alumina, &c.	68·50
	<hr/> 100·00

XI. *Notices respecting New Books.*

A Dictionary of Chemistry and the Allied Sciences. By HENRY WATTS, B.A., F.R.S. 5 vols. 8vo. London: Longmans (1863–68).

THE special literary form in which a science shall find appropriate expression is not by any means a matter of indifference. A body of truth which is replete with laws, the meet reward of meritorious centuries, may indeed, like a wealthy individual of high birth and long descent, assume the most varied exterior without discredit; but should it be scarcely yet provided with definitions, it has, like some struggler of recent lineage and narrow means, merely a single guise, which it would be impossible to improve without an awkward betrayal of the peculiar manners and partial education of the wearer. This single guise is known to literature as the *dictionary*; and in it are most fitly adduced, as belonging to a primitive epoch, the facts and circumstances on which the inductive method is to proceed. In the course of time, as hypothesis and verification mingle with their predecessors and result in law, it becomes allowable to discard in form many of the original details (whose accurate ascertainment had been nevertheless important), and to state deductively the substance of much that has been achieved. When such a period has been fairly reached, the *manual* is its legitimate exponent. Eventually, as collateral branches spring from the parent stock, a science divides itself into large (but peculiar) subjects of inquiry, and in each case the *treatise* exhibits the final results or total issue of such a proceeding.

These being, as far as we have been able to gather, the functions

which have been generally assigned to the literary forms in question, there are probably but few instances in which they have been appropriately exercised. Among very early writers the treatise is in reality a fragment of a dictionary; the manual appears too soon; and all three are confounded in a spurious "system." In modern times, the treatise partly maintains its true position and has partly degenerated into the "paper" (which is a small contribution to knowledge); the dictionary comprehends everything; and the manual is an abstract of the dictionary. It is indeed not an uncommon circumstance to find the deductive method now excluded, as far as possible, from a chemical manual; while the author introduces in its place small portions of physics, physiology, geometry, geology, or theology, which much resemble, in their mutual disconnexion and worthlessness, the scraps of information which precede a diary.

Considerations such as these cannot have escaped the learned and distinguished editor, who is in great part author, of the chemical dictionary now before us; and if he has bestowed upon his labours a collective name which we are disposed to think unsuitable, this must be attributed rather to the faults and mistaken demands of the times than to any misconception of a term.

It will be evident to anyone who will take the trouble to make the inquiry, that our English chemical dictionaries (to say nothing of those published in foreign languages) have successively exceeded their office to a continually increasing extent. Thus, in the preface to Nicholson's dictionary (to which we more especially refer, inasmuch as it was the grandparent of the present work), it is laid down, with much truth, that "when a subject does not, in itself, demand much arrangement, or if the natural arrangement be such as not to be apprehended but by those who have made considerable advances in that department of knowledge, it is evident that the utility of a dictionary to the Learner will more than compensate for the offence given to the Masters of the Science. And accordingly it is found that no systematical arrangement of mere words or terms, with their explanations, can possess so much utility as that which follows the order of the alphabet. When Macquer's Dictionary first appeared, the author remarked that Chemistry was little more than a collection of facts, scarcely entitled to the name of Science, or capable of either synthetic or analytic explanation. Whence he drew a just conclusion in favour of the Dictionary form." Nicholson proceeds to show that, even at the time he writes (1795), the argument for this form is as cogent as ever. Ure's dictionary (1821) was the immediate successor of Nicholson's. But it may be seen from the titlepage, and still more from the contents of that work, that it was the intention of the writer to diverge much from the plan adopted by his predecessor; and we are led, in somewhat fulsome phraseology, to expect "the principles of the science investigated anew, and its applications to the phenomena of nature, medicine, mineralogy, agriculture, and manufactures detailed." We have also an ingenious novelty in this book, in the shape of "an introductory dissertation containing instructions for converting the alphabetical arrangement into a syste-

matic order of study." It was attempted to carry out all these ambitious intentions in the compass of a single volume; but the professions of the author were very incompletely realized in the product of his pen. The fourth and last edition of Ure's dictionary appeared during the years 1831 and 1835, and exhibits most of the faults and merits of the first. No further edition was published; and during the last thirty years English chemical students have been accustomed to consult ill-advised "systems" of chemistry, small (and for the most part spurious) manuals, and Mr. Watts's excellent translation of Gmelin's 'Handbook' (which, its first volume excepted, is really a dictionary). There was undoubtedly a general desire that a new dictionary should be written, having the same general scope as that of Ure; and the long and arduous task has been most zealously undertaken and ably accomplished by Mr. Watts. His aim has been, we believe, to give to a reader already somewhat versed in the subject a succinct account of modern scientific chemistry and those other sciences which are situated in its immediate vicinity. Applied science constitutes no part of his design, save in its theoretical bearings; general theory and laboratory operations receive very full attention; and the editor consistently avails himself of every mode of exposition.

While we have deemed it our duty to recall the attention of authors to the formal functions of the three typical modes of scientific publication, and especially with reference to so important a work as the present, we have to regret that we are unable to give so much relative space to the discussion of its substance as has been the case with its form.

Mr. Watts's dictionary suffers little from the circumstance that its publication has extended over a series of five years, and that (as the preface to the fifth volume informs us) its preparation has been the labour of nine. Thus, for example, Dr. Odling's article "Atomic Weights," which occurs among the first, and would probably have failed, in less able hands, to indicate the transition period of chemical notation, is an excellent judicial statement of the arguments in favour of the modern principles of chemistry; and while these are illustrated with great firmness and perspicuity, the reader is very properly left to decide for himself on several points which had been raised. Neither, again, is it of very material importance that the symbolic values proposed by Gerhardt and Cannizzaro have been successively employed in these pages; and care is taken to prevent any mistake on the part of the student by making at the outset a distinction in the symbol itself. If we add to this that the nomenclature keeps pace with the notation, and is nowhere allowed to be obscure or doubtful, we have said enough to convince those for whom these volumes are intended of the editor's caution with respect to minor but significant particulars.

One of the most striking features in this dictionary is the attention which has been paid to chemical theory. Not only is this evident in the article to which we have but recently alluded, but in Professor G. C. Foster's treatment of Classification, Acid, Alkali, and
Phil. Mag. S. 4. Vol. 36. No. 240. July 1868. E

under other titles the historical development and present position of our modern hypotheses are portrayed with equal fidelity. The wisdom of such a course is sufficiently obvious, and is perfectly in accordance with the rest of the editor's plan. Good theories are now more required than the class of facts with which we are accustomed to deal. The law of multiple proportions has, fortunately for numerous investigators, enabled us to correct and employ in logic experimental results of comparatively inferior value; and it is beginning to be felt that with the accumulated labours of the last half century, some of us ought to examine anew the fundamental conceptions of chemistry. It is quite time to dispute afresh the manifold assumptions of the atomic theory, to discuss the minuter phenomena of chemical reactions, to test with rigour the philosophical stability of our definitions and notation. Few among those who, amidst the turmoil of often too hasty experiments, really care to reflect upon, and to stand face to face with, the full logical consequence of their results, will be disposed to doubt that the whole aspect of chemical theory is destined to at least as great a revolution as it has yet witnessed—a revolution in which the gross prevailing materialism will probably have to succumb, when we may expect to enjoy the freedom and purity of exact science. It is, then, most gratifying to find not only that a work like the present, whose authoritative influence will not be disputed, contains several special theoretical treatises, but that a great portion of its contents discloses quite as much theoretical as practical discussion.

Although it formed no part of Mr. Watts's plan (nor indeed would it have been desirable) to record in detail the operations of applied chemistry, there are cases in which he has very properly relaxed a rule whose rigid enforcement might have disappointed some of his readers. Gun-cotton, iron, copper, bleaching, for example, are subjects which could not be well treated at all without some reference to the manufactory; and while the student will probably learn from such articles quite as much of processes on the large scale as he desires to know, the manufacturer will find abstracts of and references to all the important memoirs which bear upon the principles he carries out.

Another point which we must not omit to notice is the fulness with which laboratory operations are described. This is particularly the case with the various branches of qualitative and quantitative analysis, the description of whose methods has been entrusted to chemists such as Mr. Conington, Drs. Dittmar and Russell, who have made this department their peculiar study. Here and there we have observed an article somewhat too compressed—that on Specific Gravity, for example; and we have noticed some omissions. We can, however, have no hesitation in attributing such deficiencies to the absolute necessity, beginning to be generally felt, that the whole should be brought to a conclusion as soon as possible; for it had been found unavoidable to depart from the original plan, which only included three volumes, and also to extend the time of publication. The mature judgment of the editor is nowhere better displayed

than in the care with which he has condensed the concluding matter and rejected the less important topics or laborious trivialities which we have noticed in other dictionaries.

In this brief review of the most extensive and valuable production of its kind which has yet appeared in the English language, the nature of our task has precluded the detailed observations which we might have felt it our duty to make in reference to a smaller book. Anything which Mr. Watts undertakes is invariably executed in so thorough, clear, and direct a manner, that his editorship and principal authorship are sufficient recommendations to any one who is acquainted with chemical literature. He has, moreover, been fortunate in obtaining the services of several distinguished chemists and physicists, who have contributed articles on subjects to which they have paid special attention. The whole work has thus the completeness and authority which are due to a well-organized literary plan well executed in all its parts; and it will probably be for many years the chief standard of reference in this country.

We venture to make, in conclusion, a recommendation which we are sure will meet with the approval of the majority of our readers. Mr. Watts's dictionary contains several distinct treatises, which have already assumed a high and distinct rank. When business requirements may permit of such a course, we shall be glad to see such articles as Heat, Electricity, and Analysis published in a separate form. Many persons who, for various reasons, do not intend to become possessors of the entire dictionary, would desire to possess one or more of those treatises, which, from their size, portability, and special character, are so very commodious for laboratory use. We are indeed persuaded (and the opinion is perhaps gaining ground) that chemistry ought earnestly to commence making the treatise its more customary form; but if this work should prove to be the last, it will certainly have been the best of its race.

Celestial Objects for Common Telescopes. By the Rev. T. W. WEBB, M.A., F.R.A.S. Second Edition, Revised and Enlarged. London: Longman and Co. 1868 (pp. 318).

The title of this work sufficiently indicates that the author's subject is not the aspects of the heavenly bodies collectively, nor their relative motions, but their aspects individually as seen through a "common telescope." A "common telescope" is defined to be one not exceeding about 5 feet in length and 4 inches in aperture. The chief contents of the work are a descriptive account of the solar system, and a list of double stars and nebulae. In both of these divisions, the author, while freely mentioning what others have made out by the use of higher powers, has kept very fairly within his limit of acting as a guide to those possessed of "common telescopes." Thus, in drawing up the list of double stars, he examined in succession with a 3·7-inch aperture all the objects in Admiral Smyth's Bedford Catalogue that there was any prospect of reaching, and retained those only "which seemed to possess sufficient general as well as scientific interest" (p. 170). To the list thus selected were added

several objects which, though unexamined, appeared to be certainly worthy of notice ; and these are very properly distinguished by being placed between brackets. The article on the moon deserves particular notice. It comprises just a fifth part of the whole work, and is an excellent sketch of “selenography.” It is accompanied by an elaborate map, slightly exceeding 12 inches in diameter, exhibiting the chief objects on the moon’s surface that common telescopes can be expected to reach. The writer has given, by way of introduction, a few remarks on the telescope, and some rules for using it, which cannot fail of being serviceable to beginners.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxxv. p. 465.]

May 7, 1868.—Dr. W. B. Carpenter, Vice-President, in the Chair.

THE following communication was read :—

“On Waves in Liquids.” By W. J. Macquorn Rankine, C.E., LL.D., F.R.S.

(1) *Object of this Paper.*—It has long been known that in a uniform canal filled with liquid, the speed of advance of a wave in which the horizontal component of the disturbance is uniform from surface to bottom is equal to the velocity acquired by a heavy body in falling through half the depth of the canal. But, so far as I know, it has not hitherto been pointed out that a similar law exists for waves transmitting a disturbance of any possible kind in a liquid of limited or unlimited depth, provided only that the upper surface of the liquid is a surface of uniform pressure. The object of this paper is to demonstrate that law, and to show some of its applications.

(2) *Velocity of Advance defined.*—Throughout this investigation the velocity of advance of a wave will be defined to be the mean between the velocities with which the shape of the wave advances, relatively to a surface-particle at the crest and to a surface-particle in the trough respectively. In ordinary rolling waves the velocities of particles in those two positions are equal and contrary, so that the speed of advance as above defined is equal to the speed of advance of the wave relatively to the earth. A wave of translation in which the velocities of particles at the crest and hollow are not equal and contrary, may be regarded as produced by compounding the motion of a rolling wave with that of a current whose velocity is half the difference of the velocities of those particles.

(3) *Relation between height of wave and horizontal disturbance at the surface.*—The following relation between the height of a wave and the horizontal disturbance of the surface-particles has already been proved and made use of by various authors ; and it is demonstrated here for convenience only. Let $+u_1$ and $-u_1$ be the velocities of a surface-particle at the crest and trough of a wave respectively. Let a be the velocity of advance of the wave as defined in article 2. Con-

ceive a horizontal current with the uniform velocity $-a$ to be combined with the actual wave-motion; the resultant motion is that of an undulating current, presenting stationary waves in its course; and the forces which act on the particles are not altered. The resultant velocity of a particle at the crest becomes $-a+u_1$; and the resultant velocity of a particle in the trough becomes $-a-u_1$. Let the height from trough to crest be denoted by Δz ; then, since the upper surface of the liquid is supposed to be a surface of uniform pressure, the principle of the conservation of energy gives the following equation:

$$g\Delta z = \frac{1}{2}\{(a+u_1)^2 - (a-u_1)^2\} = 2au_1. \quad (1)$$

(4) *Virtual Depth of Uniform Horizontal Disturbance*.—By the phrase “virtual depth of uniform horizontal disturbance,” or, for brevity’s sake, *virtual depth*, I propose to denote *the depth in the liquid to which a uniform horizontal disturbance would have to extend, in order to make the amount of horizontal disturbance equal to the actual amount*. That is to say, conceive that a pair of vertical planes normal to the direction of advance, and each of the breadth unity, coincide at a given instant, one with the trough-line or furrow, and the other with the crest-line or ridge, which bound one of the slopes of a wave. We will suppose this to be the front slope, merely to fix the ideas; for similar reasoning applied to the back slope leads to the same results. At a given depth z below the surface, let $-u''$ be the horizontal velocity with which particles are in the act of passing backwards through the plane at the trough, and $+u'$ the velocity with which particles are passing forwards through the plane at the crest; then the rate by volume at which liquid is passing into the space between those two planes is

$$\int u' dz + \int u'' dz, —$$

the integrations extending from the surface to the bottom. Let k denote the virtual depth; then

$$k = \frac{\int u' dz + \int u'' dz}{2u_1}. \quad (2)$$

(5) *Relation between Virtual Depth and Speed of Advance*.—In an indefinitely short interval of time dt , the volume of liquid which passes into the space between the two vertical planes mentioned in article 4 is

$$2ku_1 dt;$$

and in order to make room for that volume of liquid, the front slope of the wave must sweep in the same interval of time through an equal volume. But the volume swept through by the front of the wave is

$$a dt \Delta z;$$

so that, cancelling the common factor dt , we have the following equation:

$$a\Delta z = 2ku_1;$$

but, according to equation (1), $\Delta z = \frac{2au_1}{g}$; which value being substituted in the above equation, gives

$$\frac{2a^2u_1}{g} = 2ku_1,$$

and therefore

$$\frac{a^2}{g} = k, \text{ and } a = \sqrt{gk}; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

so that the *velocity of advance of a wave* (defined as in article 2) *is equal to that acquired by a body in falling through half the virtual depth*; and this is true for all possible waves in which the upper surface is a surface of uniform pressure.

(In article 6 of the paper, the speed of advance of a wave of translation is expressed by combining the speed of a rolling wave, \sqrt{gk} , with that of a supposed current, as stated in article 2.

In articles 7, 8, and 9 the law which connects the speed of advance of a wave with the virtual depth is compared with the already known laws of the transmission of rolling waves in water of limited or unlimited depth. The principal results may be summed up as follows.

Let T be the periodic time of a wave, in seconds; $h = \frac{gT^2}{4\pi^2}$ the *equivalent pendulum*—that is, the height of the pendulum whose period is the same; $c = \frac{\text{length}}{2\pi}$ the *rolling radius*, being the radius of a circle whose circumference is equal to a wave-length; u_1 the greatest horizontal velocity, and w_1 the greatest vertical velocity of a surface-particle; a the velocity of advance; then

$$a = \sqrt{gk} = \sqrt{\frac{w_1 gc}{u_1}} = \frac{w_1}{u_1} \cdot \sqrt{gh} = \frac{w_1}{u_1} \cdot \frac{gT}{2\pi},$$

and

$$k = \frac{w_1}{u_1} \cdot c = \frac{w_1^2}{u_1^2} \cdot h.)$$

(10) *Oblique Advance of Forced Waves*.—Let s be the velocity with which a floating solid body is driven horizontally; the wave which that solid body pushes or drags along with it is forced to advance at the velocity s also; while the virtual depth of disturbance, k , bears some relation to the depth of immersion and figure of the solid body. If the speed of advance corresponding to that depth, $a = \sqrt{gk}$, is less than s , a pair of wave-ridges diverge obliquely from the path of the floating body towards opposite sides; and the sine of the angle which each of those ridges makes with that path is $\frac{a}{s}$. Such is the mode of formation of the obliquely spreading waves which travel along with ships*.

* See Watts, Rankine, Napier, and Barnes, 'On Ship-building,' Division I. Article 156, p. 79.

When the velocity of the floating body is less than the speed of advance corresponding to the depth to which it disturbs the liquid in its immediate neighbourhood, it is probable that the virtual depth of disturbance of parts of the liquid beyond the immediate action of the floating body adjusts itself to the velocity, and assumes the value $\frac{g^2}{g}$.

11. *Possibility of Obliquely Advancing Tidal Waves.*—It is possible that instead of a depth less than the virtual depth corresponding to the speed of advance of a tidal wave, the ridge of that wave may place itself in a position oblique to the parallels of latitude, according to the principle stated in article 10. It still remains to be ascertained, by the study of tidal observations, whether such phenomena take place in the tides of the ocean.

12. *Terminal Velocity of Waves.*—It is known that in deep water all waves left free from the action of disturbing forces tend ultimately to assume the condition of free rolling waves, whose velocity of advance depends on their periodic time, and is expressed by the equation

$$a = \frac{gT}{2\pi}.$$

This, then, may be called the *terminal velocity* of a wave of a given period. It follows that if a wave is raised through the disturbance produced by a solid body, that wave will at first travel with a speed depending on the virtual depth of the original disturbance; but as it advances to a greater and greater distance from the disturbing body, the velocity of advance will gradually approximate to the terminal velocity corresponding to the periodic time, and the virtual depth will continually adjust itself to the changing velocity, and approximate gradually to the equivalent pendulum corresponding to the periodic time. Such is the cause of the forward curvature of the ridges of the obliquely diverging waves which follow a ship*.

May 14.—Lieut.-General Sabine, President, in the Chair.

The following communications were read:—

“Further Observations on the Spectra of the Sun, and of some of the Stars and Nebulæ, with an attempt to determine therefrom whether these Bodies are moving towards or from the Earth.” By William Huggins, F.R.S.

§ I. *Introduction.*

The author states that at the time of the publication of the “Observations on the Spectra of the Fixed Stars,” made jointly by himself and Dr. W. A. Miller, Treas. R.S., they were fully aware that the direct comparisons of the bright lines of terrestrial substances with the dark lines in the spectra of the stars, which they had accomplished, were not only of value for the more immediate purpose for which they had been undertaken, namely, to obtain information of the chemical constitution of the investing atmospheres of the stars, but might also possibly serve to reveal something of the motions

* This is explained in greater detail in a paper read to the Institution of Naval Architects on the 4th of April 1868.

of the stars relatively to our system. If the stars were moving towards or from the earth, their motion, compounded with the earth's motion, would alter to an observer on the earth the refrangibility of the light emitted by them, and consequently the lines of terrestrial substances would no longer coincide in position in the spectrum with the dark lines produced by the absorption of the vapours of the same substances existing in the stars.

The method employed by them would certainly have revealed an alteration of refrangibility as great as that which separates the lines D. They had, therefore, proof that the stars which they had examined, among others Aldebaran, α Orionis, β Pegasi, Sirius, α Lyrae, Capella, Arcturus, Castor, Pollux, were not moving with a velocity which would be indicated by such an amount of alteration of position in a line.

Since, however, a change of refrangibility corresponding to that which separates the components of D would require a velocity of about 196 miles per second, it seemed to them premature to refer to this bearing of their observations. The earth's motion, and that of the few stars of which the parallax has been ascertained, would make it probable that any alteration in position would not exceed a fraction of the change which would have been observed by them.

The author has since, for several years, devoted much time and labour to this investigation, and believes that he has obtained a satisfactory result.

He refers to Doppler, who first suggested that the relative motion of the luminous object and the observer would cause an alteration of the wave-length of the light; and to Ballot, Klinkerfues, Sonncbe, Fizeau, and Secchi, who have written on the subject.

The author is permitted to enrich his paper with a statement of the influence of the motions of the heavenly bodies on light, and of some experiments made in an analogous direction, which he received in June 1867 from Mr. J. C. Maxwell, F.R.S.

It is shown that if the light of the star is due to the luminous vapour of sodium or any other element which gives rise to vibrations of definite period, or if the light of the star is absorbed by sodium-vapour, so as to be deficient in vibrations of a definite period, then the light, when it reaches the earth, will have an altered period of vibration, which is to the period of sodium as $V+v$ is to V , when V is the velocity of light and v is the velocity of approach of the star to the earth. Equal velocities of separation or approach give equal changes of wave-length.

§ II. *Description of Apparatus.*

A new spectroscope is described, consisting in part of compound prisms, which gives a dispersive power equal to nearly seven prisms of 60° of dense flint glass. Various methods were employed for the purpose of ensuring perfect accuracy of relative position in the instrument between the star spectrum and the terrestrial spectrum to be compared with it; a new form of apparatus, which appears to be trustworthy in this respect, was contrived. Many of the ob-

servations were made with vacuum-tubes or electrodes of metal, placed before the object-glass of the telescope.

§ III. *Observations of Nebulæ.*

The author states that he has examined satisfactorily the general characters of the spectra of about seventy nebulæ. About one-third of these give a spectrum of bright lines; all these spectra may be regarded as modifications of the typical form, consisting of three bright lines, described in his former papers.

Some of these nebulæ have been reexamined with the large spectroscope described in this paper, for the purpose of determining whether any of them were possessed of a motion that could be detected by a change of refrangibility, and whether the coincidence which had been observed of the first and the third line with a line of hydrogen and a line of nitrogen would be found to hold good when subjected to the test of a spreading out of the spectrum three or four times as great as that under which the former observations were made. The spectrum of the Great Nebula in Orion was very carefully examined by several different methods of comparison of its spectrum with the spectra of terrestrial substances.

The coincidence of the lines with those of hydrogen and nitrogen remained apparently perfect with an apparatus in which a difference in wave-length of 0·0460 millionth of a millimetre would have been detected. These results increase greatly the probability that these lines are emitted by nitrogen and hydrogen.

It was found that when the intensity of the spectrum of nitrogen was diminished by removing the induction-spark in nitrogen to a greater distance from the slit, the whole spectrum disappeared with the exception of the double line, which agrees in position with the line in the nebulæ; so that, under these circumstances, the spectrum of nitrogen resembled the monochromatic spectra of some nebulæ. It is obvious that if the spectrum of hydrogen were greatly reduced in intensity, the strong line in the blue, which corresponds to one of the lines of the nebular spectrum, would remain visible after the line in the red and the lines more refrangible than F had become too feeble to affect the eye.

It is a question of much interest whether the few lines of the spectra of these nebulæ represent the whole of the light emitted by these bodies, or whether these lines are the strongest lines only of their spectra which have succeeded in reaching the earth. Since these nebulæ are bodies which have a sensible diameter, and in all probability present a continuous luminous surface, we cannot suppose that any lines have been extinguished by the effect of the distance of the objects from us. If we had reason to believe that the other lines which present themselves in the spectra of nitrogen and hydrogen were quenched on their way to us, we should have to regard their disappearance as an indication of a power of extinction residing in cosmical space, similar to that which was suggested from theoretical considerations by Chéseaux, and was afterwards supported on other grounds by Olbers and the elder Struve.

It is also shown that at the time of the observations this nebula was not receding from us with a velocity greater than 10 miles per second; for this motion, added to the earth's orbital velocity, would have caused a want of coincidence of the lines that could have been observed. If the nebula were approaching our system, its velocity might be as much as 20 or 25 miles per second; for part of its motion of approach would be masked by the effect of the motion of the earth in the contrary direction.

§ IV. *Observations of Stars.*

A detailed description is given of the comparisons of the line in Sirius corresponding to F, with a line of the hydrogen spectrum, and of the various precautions which were taken against error in this difficult and very delicate inquiry. The conclusions arrived at are:—that the substance in Sirius which produces the strong lines in the spectrum of that star is really hydrogen; further, that the aggregate result of the motions of the star and the earth in space, at the time the observations were made, was to degrade the refrangibility of the dark line in Sirius by an amount of wave-length equal to 0·109 millionth of a millimetre.

If the velocity of light be taken at 185,000 miles per second, and the wave-length of F at 486·50 millionths of a millimetre, the observed alteration in period of the line in Sirius will indicate a motion of recession between the earth and the star of 41·4 miles per second.

At the time of observation, that part of the earth's motion which was in the direction of the visual ray was equal to a velocity of about 12 miles per second from the star.

There remains unaccounted for a motion of recession from the earth amounting to 29·4 miles per second, which we appear to be entitled to attribute to Sirius.

Reference is made to the inequalities in the proper motion of Sirius; and it is stated that at the present time the proper motion of Sirius in declination is less than its average amount by nearly the whole of that part of it which is variable, which circumstance may show that a part of the motion of the star is now in the direction of the visual ray.

Independently of the variable part of its proper motion, the whole of the motion which can be directly observed by us is only that portion of its real motion which is at right angles to the visual ray. Now it is precisely the other portion of it, which we could scarcely hope to learn from ordinary observations, which is revealed to us by prismatic observations. By combining both methods of research, it may be possible to obtain some knowledge of the real motions of the brighter stars and nebulae.

Observations and comparisons, similar to those on Sirius, have been made on α Canis Minoris, Castor, Betelgeux, Aldebaran, and some other stars. The author reserves the results until these objects have been reexamined. It is but seldom that the atmosphere

is favourable for the successful prosecution of this very delicate research.

§ V. *Observations of the Sun.*

The author has observed the sun with three distinct objects in view :—

1. He has sought to discover if the spectrum of the light from the less luminous part of the sun near the limb, differs in any respect from that of the light from the central parts of his disk.

2. He hoped to obtain a view of the red prominences visible during a solar eclipse by reducing the light from our atmosphere by dispersion ; for, under these circumstances, if the red prominences give a spectrum of bright lines, these lines would remain but little diminished in brightness, and might become visible.

His observations in these two directions have been hitherto unsuccessful.

3. He proposed to seek to gain, from an examination of the spectra of the umbrae and penumbrae of solar spots, some information as to the nature of these phenomena. He has successfully applied the large spectroscope, already described, to the light from the umbra of a spot.

His observations are in accordance generally with those communicated by Mr. Lockyer to the Royal Society.

The author describes the examination of a spot on April 15th, 1868. He shows that about three-fourths of the apparent light of the umbra came from that region of the sun, and the remaining fourth from the intervening illuminated atmosphere of the earth. He observed an increase of width in most of the dark lines of the solar spectrum. The lines C and F, due to hydrogen, did not appear stronger in the spectrum of the umbra. No new lines were detected, nor were any of those of the normal solar spectrum observed to be wanting, in the spectrum of the light from the umbra. No bright lines were seen.

Some of the conditions of the solar surface are considered which the phenomena observed may be supposed to indicate.

A cooler state of the heated vapours by which the lines of absorption are produced would diminish the radiation from the gas itself, and so leave more completely uncompensated the absorption by the gas of the light from behind it. Though in this way an apparent increased intensity of the dark lines would result, the observations seem to suggest a state of the vapours, connected with tension and temperature, in which their power of absorption for each line embraces an increased range of wave-length. Some of the conditions under which this state of things may be brought about are discussed.

The absence of bright lines is not considered conclusive of the complete absence of light in the umbra from luminous gas ; for if there existed in the spot or above it the same vapours in a cooler state, the light would be almost wholly absorbed, and the feebler emanations of the cooler vapour might not do more than render less

intense the dark gaps produced by the vapours in the stronger light of all refrangibilities which is evidently present.

What is the source of the light in the umbra which gives the continuous spectrum? May the dense and intensely heated gases, which probably form the inner substance of the sun, emit, in some cases, lines so greatly expanded as to form, when numerous spectra are superposed, a sensibly continuous spectrum? Dr. Balfour Stewart has suggested that, as gases possess a power of *general* absorption of light, a heated mass of gas, if sufficiently dense to be opaque or nearly so, would give a continuous spectrum as well as the spectrum of bright lines peculiar to it.

“On the Spectrum of Brorsen’s Comet, 1868.” By William Huggins, F.R.S.

In January 1866 I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year*. I examined the spectrum of another small and faint comet in May 1867. The spectra of these objects, as far as their very feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets, the spectrum of the minute nucleus appeared to consist of a bright line between *b* and *F*, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous.

Unfavourable weather prevented me from obtaining an observation of Brorsen’s comet, at its present reappearance, before April 29. Since that evening I have examined it on May 2, 4, 6, 7, 12, 13. As I have not noticed any change in its spectrum during this time, I will put together the results of my observations on different nights, in order to avoid the repetition which would occur if the observations were arranged in the order in which they were made.

I tried various spectroscopes upon this object. The best views of its spectrum were obtained with a spectroscope of the form already described in my former papers†, and furnished with one prism of very dispersive flint glass, with a refracting angle of 60° . Some measures were taken with a similar spectroscope with two prisms of 60° .

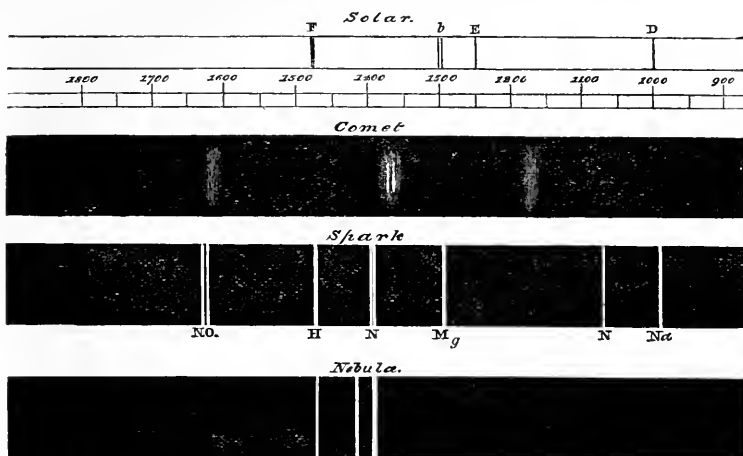
The comet appears in the telescope as a nearly round nebulosity, in which the light increases rapidly towards the centre, where on some occasions I detected, I believe, a small stellar nucleus. Generally this minute nucleus was not to be distinguished from the bright central part of the comet. I suspected two or three bright points in the coma. May 7, I perceived a small extension of the faint surrounding nebulosity in a direction opposite to the sun, so as to form a short tail.

The spectrum of this comet consists for the most part of three bright bands. The length of the bands in the instrument shows that they are not due alone to the stellar nucleus, but are produced by the light of the brighter portions of the coma.

* Proceedings of the Royal Society, vol. xv. p. 5.

† Philosophical Transactions, 1864. p. 415, &c.

I took some pains to learn the precise character of these luminous bands. When the slit was wide they resembled the expanded lines



seen in some gases—for example, the line F in the spectrum of hydrogen at the atmospheric pressure. As the slit was made narrow the two fainter bands, namely the one in the yellow and the one in the blue, appeared to fade out without becoming more defined. I was unable to resolve these bands into lines. In this respect they are very different from the bright lines of the nebulae, which become narrow as the slit is made narrow.

The middle band, which is so much brighter than the others that it may be considered to represent probably three-fourths, or nearly so, of the whole of the light which we receive from the comet, appears to possess similar characters. In this nebulous band, however, I detected occasionally two bright lines, which appeared to be shorter than the band, and may be due to the nucleus itself. This suspicion seems to be strengthened by the circumstance that when by moving the telescope the image of the comet was made to pass before the slit, these brighter lines were only observed when the middle of the comet was upon the slit; while the nebulous band continued as long as any part of the comet, except its extreme margin, was upon the slit.

Besides these three bright bands there was a very faint continuous spectrum. This spectrum is omitted in the diagram, as it could scarcely be represented without making it appear too strong relatively to the bright bands.

The position in the spectrum of the bands was determined by micrometrical measures, and also by simultaneous comparison of the bands with the bright lines of magnesium, sodium, hydrogen, and nitrogen. The brightest band, which is in the green part of the spectrum, is nearly in the position of the brightest line of the nebulae,

which coincides with the double line of the spectrum of nitrogen; but, as the diagram shows, the band in the comet is in a small degree less refrangible than the line of nitrogen. This difference of refrangibility cannot be attributed to the comet's motion, since at the time the observations were made the comet was approaching the earth.

The band in the blue is considerably more refrangible than F, and is nearly as refrangible as the group of bright lines in the air-spectrum which have the numbers 2642, 2669 in the map and tables of my paper "*On the Spectra of the Chemical Elements*"*.

The least refrangible of the bands occurs in the yellow part of the spectrum, at about the distance from E of one-third of the interval which separates E from D.

The spectrum of this comet resembles the diagram given by Donati of the spectrum of Comet I., 1864†. The positions of the three bands seen by him appear to agree with those which the bright bands of this comet occupy.

This comet differs remarkably from the two small comets which I examined, in the much smaller relative proportion of the light which forms a continuous spectrum. In Brorsen's comet, as it now appears, the bright middle part of the nebulosity seems to have a constitution analogous at least to that of the nucleus, and to be self-luminous; in the other comets the coma, which surrounded a distinctly marked nucleus, gave a continuous spectrum. The three comets resemble each other in the circumstance that the light of the bright central part was emitted by the cometary matter, while the surrounding nebulosity reflected solar light.

The telescopic observations of the heads of Donati's comet and of other large comets have shown that the luminous material is not at once driven off into the outer portions of the coma and the tail, but usually forms in front of the nucleus a dense luminous cloud, which for a time seems to be identical in the character of its light with that of the nucleus. It is, I believe, the outer portions only of the coma (which are frequently separated by dark spaces from the nucleus) and the tail which the polariscope has shown to shine by reflected light.

The positions of the bands in this comet would seem to indicate a chemical constitution different from that of the nebulae which give a spectrum of bright lines. It will be seen in the diagram that, though the brightest of the bands in the spectrum of the comet differs but little in position from the brightest line of the nebulae, the other bands are found in parts of the spectrum widely removed from those in which the other lines of the nebulae occur. The suggestion presents itself whether the broad, nebulous bands may not indicate conditions of temperature and molecular state different from those which occur in the gaseous nebulae. Plücker has shown that nitrogen and some other substances give totally different spectra under different conditions of temperature and tension. The spectrum of this comet, how-

* Philosophical Transactions, 1864, p. 158.

† Astronomische Nachrichten, No. 1488.

ever, does not resemble the other spectrum of nitrogen, which Plücker distinguishes as the spectrum of the first order*.

June 11.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

“On the Occlusion of Hydrogen Gas by Metals.” By Thomas Graham, F.R.S., Master of the Mint.

In my experiments, already published, on the occlusion of hydrogen by the metals palladium, platinum, and iron, the absorption of the gas was observed to be of uncertain occurrence at low temperatures, but was ensured by heating the metal, whether in the form of sponge or aggregated by hammering, and allowing it to cool slowly and completely in a hydrogen atmosphere. This fact was referred to the condition of absolute purity of the metallic surface being essential to the first absorbing action, as it is to the action of platinum-foil or wire in determining the combustion of the gaseous mixture of oxygen and hydrogen, as observed by Faraday. A new method of charging the metals with hydrogen at low temperatures has lately presented itself, which is not without interest.

When a plate of zinc is placed in dilute sulphuric acid, hydrogen is freely evolved from the surface of the metal, but no hydrogen is occluded and retained at the same time. A negative result was indeed to be expected from the crystalline structure of zinc. But a thin plate of palladium immersed in the same acid, and brought into metallic contact with the zinc, soon becomes largely charged with the hydrogen, which is then transferred to its surface. The charge taken up in an hour by a palladium plate, rather thick, at 12° amounted to 173 times its volume.

The absorption of hydrogen was still more obvious when the palladium plate was constituted the negative electrode in acidulated water to a Bunsen battery of six cells. The evolution of oxygen gas at the positive electrode continuing copious, the effervescence at the negative electrode was entirely suspended for the first twenty seconds, in consequence of the hydrogen being occluded by the palladium. The final absorption amounted to 200.4 volumes, and was greater in amount than the volume of hydrogen occluded by the same plate heated and cooled in an atmosphere of the gas, which did not exceed 90 volumes.

It is worthy of remark that, although the hydrogen enters the palladium and no doubt pervades the whole mass of the metal in such circumstances, the gas exhibits no disposition to leave the metal and escape into a vacuum, at the temperature of its absorption. Thus a thin plate of palladium, charged with hydrogen in the manner described, was washed, dried by a cloth, and then sealed up in an exhausted glass tube. On breaking the tube under mercury after two months, the vacuum was found perfect. No hydrogen had vaporized in the cold (about 12°); but on the application afterwards of a heat of 100° and upwards, 333 volumes of gas were evolved from the metal.

* Philosophical Transactions, 1865, p. 9.

A similar result was obtained on making a hollow palladium cylinder, of which the length was 115 millimetres, diameter 12 millimetres, and thickness 1 millimetre, the negative electrode in an acid fluid, while the closed cavity of the cylinder was kept exhausted by means of a Sprengel aspirator. No hydrogen whatever passed through into the vacuous cavity in several hours, although the gas was no doubt abundantly absorbed by the outer surface of the cylinder and pervaded the metal throughout.

It appears, then, that when hydrogen is absorbed by palladium the volatility of the gas may be entirely suppressed; and hydrogen may be largely present in metals without exhibiting any sensible tension at low temperatures. Occluded hydrogen is certainly no longer a gas, whatever may be thought of its physical condition. The same conclusion was indicated by another series of experiments, in which it was found that, to be occluded by palladium, and even by iron, hydrogen does not require to be applied under much pressure, but, on the contrary, when highly rarefied is still freely absorbed by these metals.

The occluded hydrogen is readily extracted from palladium by reversing the position of the latter in the decomposing cell of the battery, so as to cause oxygen to be evolved on the surface of the metal. The hydrogen is then drawn out as rapidly as it had previously entered the palladium, and the metal is exhausted in a complete manner by such treatment. When palladium charged with hydrogen is left exposed to the atmosphere, the metal is apt to become suddenly hot, and to lose its gas entirely by spontaneous oxidation.

Platinum may be charged with hydrogen by voltaic action, as well as palladium, but with the usual inferior proportion of gas. The charge of hydrogen taken up in a decomposing voltaic cell by old platinum in the form of a tube, of the thickness of a small crucible, was 2.19 volumes. This absorbed gas was also readily withdrawn from the platinum and oxidized on reversing the place of the metal in the decomposing cell. The platinum acquired its well-known polarizing-power in virtue of the occluded hydrogen. This power was retained by the metal after being washed with pure water and wiped with a cloth, and was brought into action on placing the metal in dilute acid. The temperature required to expel the hydrogen so absorbed by platinum was found to be little short of a red heat, although the gas had entered the metal at a low temperature.

Soft iron, left some time in a dilute acid, occluded 0.57 volume of hydrogen. This charge of gas was also retained at low temperatures, and did not escape into a vacuum till the temperature was raised nearly to redness. This proves that, like platinum, iron is not penetrated through in the cold by hydrogen, the temperature of emission being elevated considerably*.

* In M. Cailliet's experiment of exposing a thin sheet of iron to an acid, the metal is no doubt penetrated through by hydrogen in the cold, but apparently from the penetrating agency of the acid which is insinuating itself into the metal at the same time.—*Comptes Rendus*, 4 Mai 1868

While hydrogen was absorbed freely by palladium and platinum as negative plates, no oxygen whatever was absorbed by plates of the same metals in the position of positive electrodes. Oxygen gas was disengaged freely on the surface of the latter without being condensed. A platinum plate which had acted for several hours as a positive electrode, gave afterwards, when submitted to heat with exhaustion, a small trace of carbonic acid but no oxygen.

The familiar igniting-power of platinum sponge (or clean plate) upon a jet of hydrogen in the air seems to depend solely upon the influence of the metal upon its occluded hydrogen. The hydrogen appears to be polarized, and to have its attraction for oxygen greatly heightened. I beg to offer the following representation of this phenomenon, with an apology for the purely speculative character of the explanation. The gaseous molecule of hydrogen being assumed to be an association of two atoms, a hydride of hydrogen, it would follow that it is the attraction of platinum for the negative or "chlorylous" atom of the hydrogen molecule which attaches the latter to the metal. The tendency, imperfectly satisfied, is to the formation of a hydride of platinum. The hydrogen molecule is accordingly polarized, *orienté*, with its positive or "basylous" side turned outwards, and having its affinity for oxygen greatly enlivened. It is true that the two atoms of a molecule of hydrogen are considered to be inseparable; but this may not be inconsistent with the replacement of such hydrogen atoms as are withdrawn, on combining with oxygen, by other hydrogen atoms from the adjoining molecules. It is only necessary to suppose that a pair of contiguous hydrogen molecules act together upon a single molecule of the external oxygen. They would form water, and still leave a pair of atoms, or a single molecule of hydrogen, attached to the platinum.

The oxidation of alcohol, ether, and similar hydrocarbons, through the agency of platinum, likewise appears to be always an immediate consequence of a similar polarization of the hydrogen of those substances, or of some other oxidable constituent.

As has already been remarked, it does not follow that, because a gas is occluded by a metal, under the pressure of the atmosphere, at a low temperature, the gas will also escape from the metal into a vacuum at the same temperature, a much higher temperature being often required for the expulsion of the gas than for its first absorption. This is particularly true of carbonic oxide occluded by iron. Cast iron is much too porous for such experiments, and allows carbonic oxide, equally with other gases, to pass through abundantly by the agency of gaseous diffusion. Even with malleable iron there is a difficulty in observing, owing to the long time during which that metal continues to discharge carbonic oxide from its own store of that gas. But a malleable iron tube, first thoroughly deprived of its natural gas, was found to allow carbonic oxide to pass through it into a vacuum very slowly compared with hydrogen, although the volume of carbonic oxide which the metal is capable of absorbing is very sensible, amounting to 4 volumes, and more considerable than the volume of hydrogen which the same metal can occlude. Car-

bonic oxide did not sensibly pass through iron of 1·7 millimetre in thickness till the temperature was greatly elevated; and then the passage of gas was, in a minute—

Of carbonic oxide, at a full red heat, 0·284 cub. centim. per square metre of surface.

Of hydrogen oxide, at a full red heat, 76·5 cub. centims. per square metre of surface.

The condition of hydrogen as occluded by a colloidal metal may be studied with most advantage in its union with palladium, where the proportion of gas held is considerable. In the pulverulent spongy state, palladium took up 655 volumes of hydrogen; and so charged it gave off no gas *in vacuo* at the ordinary temperature, nor till its temperature was raised to nearly 100°. Hammered palladium foil has been observed to take up quite as much gas. But the condition in which palladium appears to be most absorptive is when precipitated from a solution of about 1·6 per cent. of the chloride, by the action of a voltaic battery, in form of a compact metal. Palladium is not one of the metals readily thus precipitated; but it may be thrown down upon a thin platinum wire, in brilliant laminæ, by the action of a large single cell. The palladium after a time detaches itself from the wire, exhibiting a bright white metallic surface where it had been in contact with the platinum, and a dull surface, suggesting metallic arsenic, on the side exposed to the acid. As so prepared, it does not contain any occluded hydrogen. But the metallic films, when heated to 100° in hydrogen, and allowed to cool slowly for an hour in the same gas, were found to occlude 982·14 volumes of gas, measured with thermometer at 11°, and barometer at 756 millimetres. This is the largest absorption of hydrogen observed. From palladium so charged there was a slight indication of the escape of hydrogen into a vacuum, with extreme slowness in the cold. This charged palladium is represented by weight as

Palladium 1·0020 grm.	99·277
Hydrogen 0·0073 grm.	·723

100·000

It is in the proportion of one equivalent of palladium to 0·772 equivalent of hydrogen*, or there is an approximation to single equivalents Pd H. But the idea of definite chemical combination is opposed by various considerations. No visible change is occasioned to the metallic palladium by its association with the hydrogen. Hydrides of certain metals are known, as the hydride of copper (Wurtz) and the hydride of iron (Wanklyn); but they are brown pulverulent substances with no metallic characters. Indeed a hydride of palladium itself can be formed, but not preserved, on account of its great instability. Following the process of M. Wurtz for the hydride of copper, nitrate of palladium was boiled with sulphuric acid, and the sulphate of palladium (a red crystalline salt)

* H=1, Pd=106·5.

prepared. A solution of this salt, with an excess of sulphuric acid, was precipitated by the hypophosphite of soda; a black powder fell, which speedily underwent decomposition at 0° , evolving copious volumes of hydrogen gas. The final residue appeared to be pure palladium, of its usual black amorphous appearance, and with no trace of crystallization. It is singular that this palladium precipitate contained no occluded hydrogen; and even when heated, and afterwards exposed to an atmosphere of hydrogen in the usual manner, the palladium black so prepared condensed no sensible quantity of that gas.

I am inclined to conclude that the passage of hydrogen through a plate of metal is always preceded by the condensation or occlusion of the gas. But it must be admitted that the rapidity of penetration is not in proportion to the volume of gas occluded; otherwise palladium would be much more permeable at a low than at a high temperature. A plate of that metal was sensibly exhausted of hydrogen gas at 267° , but continued permeable, and in fact increased greatly in permeability at still higher temperatures, and without becoming permeable to other gases at the same time. In a striking experiment, a mixture of equal volumes of hydrogen and carbonic acid was carried through a small palladium tube, of which the internal diameter was 3 millimetres, and the thickness of the wall 0.3 millimetre. From the outer surface of this tube gas escaped into a vacuum, at a red heat, with the enormous velocity of 1017.54 cub. centims. per minute for a square metre of surface. This gas did not disturb baryta-water. It was pure hydrogen.

A still more rapid passage of hydrogen was observed through the substance of a hollow cylinder of palladium 1 millimetre in thickness, at a higher temperature, approaching the melting-point of gold. The palladium cylinder being enclosed in a porcelain tube charged with pure hydrogen, was exhausted as usual, and gave 105.8 cub. centims of gas in five minutes; measured with bar. 753 millims., therm. 10° . As the external surface of the palladium tube amounted to 0.0053 square metre, the passage of gas was

3992.22 cub. centims. from a square metre of surface, per minute.

The rate of penetration of hydrogen through the same palladium tube, at the lower temperature of 265° C., was previously observed to be

327 cub. centims. from a square metre of surface, per minute.

The velocity of penetration thus appears to increase in a rapid ratio with the temperature.

When carbonic acid was substituted for hydrogen, at the same high temperature, a very minute penetration was perceived, amounting to

1.86 cub. centim. from a square metre of surface, per minute.

This gives for carbonic acid one twenty-thousandth part of the rate of hydrogen. Whether it is a penetration of the same sort, although greatly less in degree, or rather the consequence of a sensible porosity in the palladium (of which it would become the measure), remains uncertain.

The quantity of hydrogen held by the metal at these high temperatures may become too small to be appreciated; but I presume it is still present, and travels through the metal by a kind of rapid cementation. This extreme mobility is a singular property of hydrogen, which was involved in the fundamental discovery, by MM. H. Sainte-Claire Deville and Troost, of the passage of that gas through plates of iron and platinum at high temperatures.

The marked rapidity of the passage of the same gas through a thin sheet of caoutchouc appears to be more capable of explanation on known principles. Caoutchouc of less than 0.1 millimetre in thickness, if impregnated with hydrogen, loses its gas entirely by the most momentary exposure to the air. A tube of 2 millimetres in thickness, through which hydrogen and carbonic acid were singly passed, each for an hour, was found to retain—

Of hydrogen	0.0113 volume.
Of carbonic acid	0.2200 „

The absorption, then, is in the proportion of 1 hydrogen to 20 carbonic acid, but the comparative rate of penetration of the two gases through a sheet of caoutchouc is as 1 hydrogen to $2\frac{1}{2}$ carbonic acid; or the hydrogen moves eight times as rapidly as the density of its solution would indicate. But these gases differ in diffusibility as carbonic acid 1 to hydrogen 4.7. The rapid passage of hydrogen through caoutchouc is thus partly explained by the rapid manner in which that gas is brought to one surface of the sheet and conveyed away from the other by gaseous diffusion. Again, both substances travel through the substance of the caoutchouc by their diffusibility *as liquids*. Suppose hydrogen in that form to be nearly as much more diffusive than the other substance as it is when both are gaseous, then the observed rapid passage of hydrogen through caoutchouc would appear to be fully accounted for.

Liquid diffusion has also a bearing upon the rapid dissemination of hydrogen through a soft colloid metal, like palladium or platinum, at a high temperature. The liquid diffusion of salts in water is known to be six times as rapid at 100° as at 0° . If the diffusion of liquid hydrogen increases with temperature in an equal ratio, it must become a very rapid movement at a red heat. Although the quantity absorbed may be reduced (or the channel narrowed), the flow of liquid may thus be increased in velocity. The whole phenomena appear to be consistent with the solution of liquid hydrogen in the colloid metal. The “solution affinity” of metals appears to be nearly confined to hydrogen and carbonic oxide, so that metals are not sensibly penetrated by other gases than these.

ROYAL INSTITUTION OF GREAT BRITAIN.

May 22, 1868.—“On some Effects of the Heat of the Oxyhydrogen Flame.” By William Odling, M.B., F.R.S.

I.

Chemical changes, whether of combination or decomposition, result in the production of new bodies which, under the conditions

of the change, have for the most part a greater stability than the original bodies.

One evidence of this greater stability is afforded by the development of a quantity of heat (the heat of chemical action) from the produced bodies having a smaller potential heat than the original ones.

It results, both from reason and experiment, that in order to undo or reverse any definite chemical action, just so much heat must be directly or indirectly expended as was evolved by the original action.

For the same quantity of heat evolved, the resulting temperature varies with the mass and kind of matter heated, and with the rapid or gradual evolution of the heat.

When the evolution of heat is instantaneous, the resulting temperature may be calculated from the quantity of heat evolved and the mass and specific heat &c. of the matter heated.

By a unit of heat is meant the quantity of heat necessary to raise the temperature of one kilogramme of water one degree Centigrade, or, more accurately, from 0° to 1° .

II.

Every 18 grammes of water is a combination of two 1-gramme proportions of hydrogen H, with one 16-gramme proportion of oxygen O; and by the combination of 2 grammes of hydrogen with 16 grammes of oxygen, there are developed 68 units of heat.

Of these 68 units of heat, however, little more than 57 units are really due to the chemical action, nearly 11 units of heat being evolved by the contraction of the original mixed gas into two-thirds its volume of steam, and by the further condensation of the resulting steam into 18 cubic centimetres of water.

While the quantity of heat evolved by the combination of a given quantity of oxygen and hydrogen is invariable, the intensity of the heat may vary from a scarcely recognizable rise of temperature up to the highest temperature of the oxyhydrogen blowpipe flame, capable of fusing platinum and silica.

A most remarkable effect of the intense temperature resulting from the combination of oxygen and hydrogen into water, is the partial decomposition of water into oxygen and hydrogen, discovered by Mr. Grove in 1846.

At this high temperature hydrochloric acid and carbonic anhydride gases also undergo partial decomposition, into hydrogen and chlorine and into carbonous oxide and oxygen respectively.

Upon what do these singular decompositions by heat, of bodies formed with great evolution of heat, depend? or with what class of chemical phenomena may they be associated?

III.

Under certain familiar conditions, chemical action seemingly takes place to its utmost possible extent in a single direction only, with production of a maximum amount of the substance that is formed with maximum evolution of heat.

For example, taking atomic proportions in grammes, the heat of formation of chloride of zinc, ZnCl_2 , is 101 units; and the heat of

formation of chloride of copper, CuCl_2 , is 60.5 units. Hence, with chlorine in solution and excess of both copper and zinc, there is finally produced the maximum possible amount of chloride of zinc and no chloride of copper.

Again, on addition of sufficient zinc to solution of chloride of copper, there is complete combination of chlorine with zinc and complete separation of chlorine from copper, *i. e.* complete burning of the one metal and complete unburning of the other.

IV.

But under simpler though less familiar conditions, chemical action habitually takes place in more than one direction simultaneously, with production of correlative products in varying proportions.

Thus, with hydrogen and excess of both chlorine and oxygen, although the heat of formation of oxide of hydrogen, H^2O , is 57 units, and the heat of formation of chloride of hydrogen, 2HCl , is only 47.5 units, yet in this case the hydrogen does not combine with the oxygen to the exclusion of the chlorine, but divides itself between the oxygen and the chlorine in proportions which vary with the conditions of the experiment.

In accordance with this result it is found that, at the same red heat, excess of chlorine will effect the partial decomposition of water with extrusion of oxygen, and, conversely, that excess of oxygen will effect the partial decomposition of hydrochloric acid with extrusion of chlorine.

So that, beginning with the two chemical substances water and chlorine, or beginning with the two chemical substances hydrochloric acid and oxygen, or beginning with the three chemical substances hydrogen, chlorine, and oxygen, there exist at a full red heat the four chemical substances, water, hydrochloric acid, chlorine, and oxygen—the proportions of the four substances depending certainly upon the relative quantities present of the elements concerned, and most probably also upon the temperature of the experiment.

Similarly, beginning with the one chemical substance water (Grove), or beginning with the two chemical substances oxygen and hydrogen (Bunsen), there always exist at a sufficiently high temperature the three chemical substances water, oxygen, and hydrogen.

Although, by exposure to a red heat, the electrolytic mixture of oxygen and hydrogen gases becomes completely combined or transformed into water, yet, as recently shown by Bunsen, at the high temperature of 2024° only one-half, and at the still higher temperature of 2844° only one-third of the mixture undergoes combination, the other one-half or two-thirds remaining in the state of mixed gas.

V.

Chemists are acquainted with many reciprocal actions comparable with those of chlorine upon water, and of oxygen upon hydrochloric acid, the most familiar instance being probably the decomposition of ignited oxide of iron by hydrogen with extrusion of iron, and the converse decomposition of oxide of hydrogen by ignited iron with extrusion of hydrogen.

Similarly, sodium will decompose the oxides of carbon, while carbon will decompose oxide of sodium; and just as a sufficient excess of chlorine may be made to effect the almost complete decomposition of a given quantity of water, so may a sufficient excess of carbon (or carbonous oxide) be made to effect the almost complete decomposition of a given quantity of sodium-oxide or zinc-oxide, as in the ordinary processes for obtaining the two metals, notwithstanding that, for an equal consumption of oxygen, the respective combination heats of sodium and zinc exceed by far the combination heat of carbon or carbonous oxide.

Again, although the combination heat of oxygen and carbonous oxide is 68 units, while that of oxygen and hydrogen is only 57 units, yet, as was shown by Bunsen many years ago, upon exploding a mixture of oxygen with a joint excess of carbonous oxide and hydrogen, the oxygen does not attach itself exclusively to the carbonous oxide, but divides itself between the carbonous oxide and hydrogen in a ratio determined by their relative proportions.

GEOLOGICAL SOCIETY.

[Continued from vol. xxxv. p. 399.]

March 25th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "On some new species of Palæozoic Crustacea from the Upper Silurian rocks of Lanarkshire, &c., and further Observations on the Structure of *Pterygotus*." By Henry Woodward, Esq., F.G.S., F.Z.S.

The nature of the remains which have been referred by Mr. Salter to *Eurypterus* (*Pterygotus*) *punctatus* was first discussed by the author, who came to the conclusion that the Lanarkshire specimens belong to a new species—*Eurypterus scorpioides*,—while the chelate antennæ and the detached lip-plate must have belonged to other species.

Eurypterus scorpioides is the first of the new forms now described by Mr. Woodward, and is represented by a specimen exhibiting an almost entire individual, and certain other fragments. The punctate ornamentation of this species may be readily distinguished from the scale-like markings of *Pterygotus* and *Slimonia*. The second new form, *Eurypterus obesus*, is remarkable for the great obesity of the thoracic somites; it is represented by the impression and counterpart of an entire specimen. Its small size suggested to the author the possibility of its being the young of some larger species. The third new species, *Pterygotus raniceps*, is at present known only by a single example; its head is remarkable on account of its obtusely pointed triangular form and prominent marginal eyes.

In conclusion the author made some observations on the structure of *Pterygotus*, showing that it possessed a series of branchial plates—leaf-like bodies presenting a highly vascular and delicate struc-

ture, arranged in a linear series of from 6 to 8 in each row, and appearing to have occupied a position beneath the thoracic plate on the ventral surface of the body, as seen in *Limulus* at the present day. He also suggested that *Pterygotus perornatus* and *P. crassus* are both varieties of *P. bilobus*.

2. "On the Coniston Group." By Prof. R. Harkness, F.R.S., F.G.S., and Dr. H. A. Nicholson, F.G.S.

The object of this communication was to record the occurrence of a new and unique horizon, containing a rich Graptolite-fauna, in that portion of the Silurian series of the Lake-district termed the Coniston-flags by Professor Sedgwick. The authors also gave a detailed description of these flags, and pointed out their physical and palæontological relations with the Coniston Limestone below and the Coniston Grits above them.

The palæontological relations of the Coniston Limestone and of the underlying green slates and porphyries have been previously shown to be those of the Bala and Caradoc group. The mudstones succeeding to the Coniston Limestone yield an entirely new fauna, including six species of *Diplograpsus*, all of which, with one exception, are in Britain characteristic Upper-Llandeilo forms; and the evidence of the other species is in the same direction. In Ireland, however, many of these species have been obtained from strata of Caradoc age. The fossils of the Coniston Grits have very little affinity with those of the Kendal Flags, nor do they exhibit such a facies as would connect them with the lower members of the Upper Silurian series. Palæontologically, therefore, this Coniston series must be looked upon as a continuous group of rocks; and the physical evidence leads to the same conclusion. There is thus, in the Lake-district, a greater development of Caradoc and Bala rocks than is to be found elsewhere in the British Islands, as we are now required to add a great thickness of strata, possessing, on the whole, a decidedly Lower-Silurian fauna, but containing some new forms of life in its higher portions.

3. "Death of Fishes on the coast of the Bay of Fundy." By Dr. A. Leith Adams, F.G.S., 22nd Regiment.

On the 24th of September, during a heavy gale from the west, impinging almost straight on to the entrance of the Lagoon, known as Anderson's cove, enormous numbers of fish were observed floating dead upon the surface of the water, and thrown up in quantities by the waves. On the gale subsiding, the whole surface of the lagoon and its banks were covered with dead fish, to the depth of a foot in some places. It was evident that the shoal had been literally ground to pieces against the rocks by the force of the waves. In conclusion the author referred to the vast quantities of fossil fish found in the Devonian and other strata, which suggested catastrophes allied to the above incident.

4. "On Volcanoes in the New Hebrides and Banks Islands." By R. Atkins, Esq., of the 'Southern Cross.'

The author described the islands of Tanna, Lopevi, and Ambrym,

in the New Hebrides, and Santa Maria and Great Banks Islands, among the Banks-Island group, as being now active volcanoes, and gave an account of a visit to the Hot Springs of Great Banks Island. These springs deposit quantities of almost pure sulphur.

April 8th, 1868.—Prof. T. H. Huxley, LL.D., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "On the Affinities and probable Habits of the extinct Australian Marsupial, *Thylacoleo carnifex*, Owen." By W. H. Flower, Esq., F.R.S., F.G.S., &c.

Thylacoleo was first described by Prof. Owen in the Philosophical Transactions for 1859, from an imperfect skull, the characters of which led to the conclusion that it was "one of the fellest and most destructive of predatory beasts," having its nearest affinities among existing marsupials with *Dasyurus ursinus*, although the interval be still very great between them. In a subsequent description of a more perfect skull, Prof. Owen's views of the affinities, though not of the habits and food of the animal, were modified. It was stated to be more nearly related to the Diprotodons, Nototheres, Koalas, Phalangers, and Kangaroos, but at the same time to exemplify "the simplest and most effective dental machinery for predatory life and carnivorous diet known in the Mammalian class."

The author of the present paper, while entirely concurring with Prof. Owen in his later views of the affinities of *Thylacoleo*, and pointing out in detail its relations, especially with the Rat Kangaroos (*Hypsiprymnus*) and the Phalangers (*Phalangista*), demurred to the soundness of the conclusion as to its predaceous habits. He remarked that as the greater number, if not all, of the known animals of the group to which *Thylacoleo* undoubtedly belongs are either vegetable or mixed feeders, the probabilities would be that this creature conformed with its congeners in this respect, unless it possessed any such striking adaptive modification of the normal typical dentition of the group as to lead to a directly opposite conclusion.

He then proceeded to discuss this question, showing that in its rodent-like incisors, rudimentary canines, and hypsiprymnoid premolars it presents no sufficient approximation to any of the true predaceous carnivores, either placental or marsupial, as in his opinion to justify the inference as to its habits which is expressed in the name bestowed upon it.

2. "On the Thickness of the Carboniferous Rocks of the Pendle Range of Hills, Lancashire." By E. Hull, Esq., B.A., F.R.S., F.G.S., of the Geological Survey of Scotland.

This paper was supplementary to a former communication by the author, in which he endeavoured to prove the south-easterly attenuation of the Carboniferous *sedimentary* strata of the North of England, while the *calcareous* member (the Mountain-limestone) attained its

greatest vertical development in Derbyshire, and thence thinned away northward and westward. The author now gave the results of his subsequent investigations while engaged in the survey of the Pendle Range and the neighbourhood of Burnley and Blackburn, which have shown that the increase in the thickness of the *sedimentary* deposits is continued into that district, the aggregate thickness of the Coal-measures, the Millstone-grit, and the Yoredale series being in the Burnley district 18,635 feet, while in Leicestershire it has dwindled down to 3100 feet. In discussing the question of the source of these sediments, the author came to the conclusion that they were derived from a primæval Atlantis,—a view which he considered to be strengthened by the fact that the Carboniferous sedimentary strata of North America also swell out towards the north-east, and become attenuated towards the south and west.

3. "Observations on the relative Ages of the leading physical Features and Lines of Elevation of the Carboniferous district of Lancashire and Yorkshire." By E. Hull, Esq., B.A., F.R.S., of the Geological Survey of Scotland.

The author first described the Pendle Range as a great arch of Carboniferous rocks, bordered on the north and south by a succession of parallel (W.S.W. to E.N.E.) arches and troughs, to all of which he assigned a Præpermian age. He regarded them as belonging to the earliest of three consecutive periods of disturbance, to which all the principal flexures and faults of the district may be referred. The Pennine Chain, which runs nearly north and south, he believed to have been upheaved during a later period, namely, the close of the Permian, while the numerous north-west faults of the district under consideration he referred to the close of the Jurassic period. Mr. Hull described in detail the evidence upon which these conclusions rested, observing that immediately upon the close of the Carboniferous period the northern limits of the Lancashire and Yorkshire coal-fields were determined by the upheaval and denudation of the beds along east and west lines, the coal-fields themselves retaining their original continuity across the region now formed of the Pennine Hills, from Skipton southwards. At the close of the Permian period these coal-fields were dissevered by the uprising of the area now formed of the Pennine range, by lines of upheaval ranging from north to south, nearly at right angles to the former, this fact being of itself an evidence of difference of age. In conclusion the author pointed out that the denudation of the rocks of the district may be referred to seven periods, beginning with the commencement of the Permian and ending with the Postglacial; he defined the duration and effect of each of these periods, and stated the evidence on which his conclusions rested.

XIII. *Intelligence and Miscellaneous Articles.*

ON THE SPECTRUM OF BRORSSEN'S COMET.

BY FATHER SECCHI.

TAKING advantage of a few tolerably bright evenings, I have examined the prismatic spectrum of Brorsen's comet, which is now tolerably visible.

The spectrum is not visible enough with compound spectroscopes with a slit, the light being too feeble; I therefore used a simple spectroscope with direct vision. To fix the position of the lines, I first used the direct image of the comet seen with its spectrum; but as the light was still too faint, I determined the relative positions of the lines by comparing them with those of Venus, placing the comet and the planet successively in the same position on the finder. Both methods gave identical results, but the latter gave most light.

The spectrum of the comet is discontinuous; it consists at first of feeble light, filling the field of view, in which three zones stand out with sufficient distinctness to appear more expanded than the rest. The brightest is the middle zone, which occupies the green colour, and corresponds to the region comprised between the magnesium (*b*) and the hydrogen (F), but much nearer the former; the breadth of this zone is very restricted, and does not exceed one-fifth of the distance of the two lines. When the atmosphere is particularly favourable, it is almost reduced to a single bright line, about as broad as the nucleus of the comet. Another brilliant zone, though far less intense, is met with in the greenish yellow, in the middle of the distance between the sodium (D) and the magnesium (*b*). Another band in the red is sometimes met with, but it is fixed with difficulty. The third zone of luminous intensity, about halfway between the two former, is on the side of the blue, at about one-third of the distance from F to G, starting from F. This band is brilliant enough to be well measured, and produces by scintillation the appearance of a line.

The following is the relative position of these bands referred to the spectrum of Venus in terms of the micrometer:—

Sodium (D)	5·13
Comet, first luminous band.....	5·92
Magnesium (<i>b</i>)	6·83
Comet, second, very brilliant band..	7·07
Hydrogen (F)	7·94
Comet, last, rosy line	8·52
Line G	10·87

These measurements are as exact as the feebleness of the comet's light permits, which is scarcely that of a star of the seventh magnitude.

These observations lead us to interesting results. It seems at

first that we are justified in concluding the light to be formed solely of reflected solar light ; now that which comes from the sun would perhaps only constitute the diffused basis of the field of vision. The comet would then have a light of its own, the tint of which very much resembles that of the nebulae, but its position greatly differs from that of the nebular lines, one of which coincides with *f*. The other line of the comet is also different in position, and is on the further side, nearer magnesium than the nebular ray. It is thus proved that the displacement does not result from motion, but is due to the nature of the cometary matter. As these bands are more luminous than those of a star of equal magnitude, we are led to the assumption of a special light emanating from the comet. The measurements are not close enough to justify our comparing these spectra with those of other known spectra ; and we now know, moreover, that this comparison would be illusory, for the visibility of the spectra in gases depends on numerous circumstances which we cannot determine.

In the second place, this spectrum strongly resembles that which other astronomers besides myself have observed in other comets. We may thus generalize these results by induction ; and that would furnish an argument for the extraplanetary origin of these bodies.

Taking Venus near its greatest digression, and analyzing it with a spectroscope with angular vision with two prisms, I observed that the light indicated traces of atmospheric action at a height above the horizon too great for the influence of the terrestrial atmosphere to be perceptible. This must then be referred to the atmosphere of Venus itself. It is well seen in the region of Brewster, named δ , and in the other region in front of the sodium. These bands are due to aqueous vapour. The lines of nitrogen of the air near F are also greatly strengthened, which proves the presence of an atmosphere about this planet analogous to our own.

These observations have been confirmed on the simple spectroscope made with Merz's and with Hofmann's prisms, using eyepieces formed with cylindrical lenses, which were sent me by M. Merz, and which possess a high degree of perfection.

Now that with such powerful means we can penetrate into the constitution of the stars, I have put the question to myself whether the diversity of the spectra in certain types of stars does not rather arise from differences in the proportions of substances which constitute them, than from an absolute diversity in the substances themselves. I have commenced this investigation in detail by comparing stars of the second type (yellow stars like our sun) and red stars of the third type.

Comparing the two stars α Orionis and Arcturus, a surprising identity is found between the principal lines and those of our sun. More than thirty lines were found to be identical with those of Fraunhofer's map, which by its size is better fitted for these comparisons than Kirchhoff's large map. The fundamental difference appeared to consist in the strongest lines being more intense in

α Orionis than in Arcturus; and all are less so than in the sun. It might, moreover, be asked whether even in our sun the intensity of these lines is constant; and this we are led to doubt when we compare Fraunhofer's figure with that of Van der Willigen, in which fainter lines are met with than those marked by the first observer. The group which stands out the best is that of iron; α Orionis has, moreover, systems of gaseous absorption analogous to those of our atmosphere. But this kind of investigation is only just sketched out, and much time is required to arrive at definite results.

Unfortunately this investigation cannot be accelerated, owing to the small number of nights without any moon, and suitable for it in other respects, and to the danger of fatiguing the sight too much. A phenomenon is in fact frequently met with which shows how an observer must be somewhat on his guard. When, after having kept the eye fixed on a star for some time, we look into the finder of the large telescope for several seconds, a perfectly dark band of the same size as the spectrum is seen across the field itself. The eye is thus paralyzed by the decomposed light far more than by a white light of equal intensity. Inversely, on putting the eye to the telescope, for the first few moments scarcely any of the numerous details are observed, which begin to come out little by little until complete definition is attained. These phenomena show that the eye must be spared, and that without care it might be injured in these researches.

But at the same time we see what an intensity the system of cylindrical ocular lenses can retain. Some time ago it could scarcely have been suspected that the eye could be fatigued by the light of the spectrum of a star.—*Comptes Rendus*, May 11, 1868.

ON GILDING GLASS FOR THE PREPARATION OF OPTICAL MIRRORS.
BY W. WERNICKE.

In the last ten years various methods of silvering glass for the preparation of optical mirrors have been described, which have been successfully applied in practice. In order also to enfeeble the action of the sun's rays in solar observations, it is usual now to coat the object-glasses of telescopes with thin layers of silver—a method first proposed by Foucault, and now introduced into several observatories. The nature of silver is opposed to a continued use of the metallic mirror; for when exposed to the air it loses its lustre after no great length of time, owing to the foreign gases present in the air.

Hence it has been frequently proposed to replace silver by gold for the above purposes; but the methods published have turned out to be of little use when tried. In vol. cxi. of Liebig's *Annalen*, Liebig has described a method based on the reduction of a specially prepared alkaline solution of gold by a mixture of alcohol and ether. The success of this method depends, however, on points not fully understood; and it is therefore inapplicable on a large scale.

Engaged on optical experiments, I have recently found a method which never fails and can always be easily and conveniently executed. For the preparation of a brilliant and firmly adhering layer of gold three solutions are prepared, which are kept separate, and when used need only be mixed in definite proportions:—

1. A solution of chloride of gold in water containing 1 grm. gold in 120 cubic centims. The gold is dissolved in the smallest quantity of aqua regia, the excess of acid evaporated on the sand-bath, and the solution diluted 120 cubic centims. It is not necessary to heat the hydrochloric acid solution until protochloride is formed; for the presence of a small trace of free acid is unimportant in the formation of a good mirror. The solution of gold must, however, be absolutely free from such metals as are deposited in the metallic form by the reducing-liquid, and more especially from silver. If the chloride of gold contains traces of chloride of silver, most of the gold is deposited in the metallic form, and the thin badly coloured mirror is soon detached from the glass.

2. Soda-lye of the specific gravity 1.06. This need not be pure; in my experiments I have used commercial soda (containing chlorine and sulphuric acid) made caustic by ordinary lime, with the same success as chemically pure caustic soda.

3. The reducing-liquid. 50 grms. of sulphuric acid are mixed with 40 grms. of alcohol and 35 grms. of water; and after the addition of 50 grms. of finely powdered manganese, the mixture is distilled at a gentle heat on the sand-bath, and the vapours passed into a flask containing 50 grms. of cold water. The distillation is continued until the volume of the water is doubled; the liquid obtained, which contains aldehyde along with some formic and acetic ether, is mixed with 100 cubic centims. of alcohol and 10 grms. of sugar converted by means of nitric acid. The change of the sugar is effected by dissolving 10 grms. of ordinary cane-sugar in 70 cubic centims. of water, adding 0.5 grm. of nitric acid of the specific gravity 1.34, and boiling for a quarter of an hour. This reducing-liquid, preserved in well-corked bottles, may be used for several months with the same success.

In order to prepare a plane or a concave mirror, one part of the soda-lye is mixed in a suitable vessel with four times its volume of the solution of gold, and a quantity of reducing-liquid amounting to $\frac{1}{3\frac{1}{5}}$ or at most $\frac{1}{3\frac{1}{10}}$ of the whole is added. The mixture is rapidly coloured green, owing to the liquid separated; it is immediately brought in contact with the surface to be gilded, and in such a manner that the gold can deposit from the top downwards. The rapidity of the gilding depends on the temperature. With a mean temperature of 18° C. the mirror begins to form in 30 minutes, in 1½ hour it is transparent with a splendid green colour, and in 2½ to 3 hours it is so thick that it is only translucent with a dark-green light. At a temperature of 50° to 60° C. the same effect takes place in 20 to 25 minutes, and at a temperature of 75° even more rapidly; it is not desirable to use a higher temperature, for the gold appears then to adhere less firmly to the glass. Within the limits mentioned,

the excellence of the mirror is the same as regards lustre and durability; but it may sometimes happen that at a high temperature the air contained in the water ascends in small bubbles and thus causes fine holes in the mirror, which are not visible in reflected, but are so in transmitted light; for this reason, when a high temperature is used, it is convenient to raise nearly to boiling the alkaline solution of gold before the addition of the reducing-liquid, and before bringing it in connexion with the glass to be gilded.

The mirror, when ready, is carefully washed with water, leaned against the wall with the bottom resting on bibulous paper and the coated side underneath, and dried in the air at the ordinary temperature; it exhibits then a perfect polish.

In preparing the glasses for gilding, it is in most cases sufficient to clean the surface with soda-lye and alcohol. Care must be taken not to use an acid for cleaning; the layer of gold in this case subsequently becomes detached from the glass.—Poggendorff's *Annalen*, January 1868.

ON SOME SPECIAL ELECTROMAGNETIC PHENOMENA, AND ON WEBER'S HYPOTHESIS ON ELECTROMAGNETISM. BY E. VILLARI.

The author has investigated the influence which the initial magnetic state of a bar of iron or of steel exerts on induced magneto-electrical currents excited in an adjacent conductor by any modification whatever, be it increase or diminution, in the magnetic condition of the bar. His object was to verify Weber's theory, which was also proposed by De la Rive, and which is only a complement of that of Ampère, according to which, molecular currents preexist in magnetic bodies, and are merely directed by the action of magnetization, carrying along with them in this motion the particles of the body. He thinks that if there is molecular motion, the rapidity with which it is effected must depend on the initial magnetic condition, like the magneto-electrical currents which result from it, and which themselves depend on the rapidity of the magnetic modification.

The apparatus he used consisted of a bar of steel or iron, round which were coiled two conducting wires; through the outside one a first current passed, which magnetized the bar, and then other weaker ones, direct or inverse, which more or less modified the original magnetism; in the other, the inside, which was connected with a galvanometer, the breaking of the outside coil and the alteration in the magnetism produced induced currents, which were to be measured.

The author himself gives the following summary of his researches, which, in his view, fully confirm Weber's theory:—

(1) Magnetization and demagnetization are accompanied by molecular movements.

(2) The rapidity with which these molecular motions are effected depends on the initial molecular condition of the iron or the steel.

(3) The magneto-electrical current is not always proportional to the increase or diminution of the magnetic moment of the magnet; it depends not only on this quantity, but also on the rapidity with which the magnetic modification is effected.

(4) This modification is not always proportional to the intensity of the current which produces it, even taking into account the magnetic capacity of the steel or iron bar in which it is produced.—Poggendorff's *Annalen*, vol. cxxxiii. p. 233; *Bibliothèque Universelle*, May 15, 1868.

APPLICATION OF THE VIBRATIONS OF COMPOUND BARS TO DETERMINING THE VELOCITY OF SOUND. BY J. STEFAN.

The method introduced by Chladni of determining, from the longitudinal tones of bars, the velocity of sound in these bars, is not applicable to those bodies which have not the form of long bars, or cannot by friction be made to sound. For such cases the new process serves.

The body to be investigated is constructed in the shape of a small bar, and fastened on a longer bar of wood or glass, which can itself be readily made to sound. This compound bar may now be made to sound by friction, and thus the number of vibrations of the fundamental note or of a higher tone be determined. Knowing, then, the velocity for the longer bar, that for the smaller one may be deduced by a formula which, it is true, is somewhat complicated.

By this method the following observations, among others, were made:—

The velocity of sound in wax = 730 metres, and is thus a little more than twice as great as in air. This number refers to the temperature 20° C.; with increase in temperature the velocity of sound decreases so much that a decrease of 40 metres corresponds to an increase of 1°, and at 30° the velocity of sound in wax and in air are the same.

The velocity of sound in grease is at 20° C. only half as great as in wax, and with an increase in temperature diminishes somewhat more quickly than in wax.

For the velocity of sound in caoutchouc, values were found which fall between 30 and 60 metres; the softer the caoutchouc, the less the velocity of sound.

These results recall those of Helmholtz on the velocity of the propagation of nervous excitation, which are within the same limits as the velocity of sound in soft caoutchouc, and suggest the thought that the velocity of nervous excitation coincides with that of sound, and that nervous excitation can be propagated in longitudinal waves.—*Berichte der Kaiserlichen Akademie in Wien*, April 30, 1868.

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[FOURTH SERIES.]

AUGUST 1868.

XIV. *Experimental Researches in Magnetism and Electricity.—*
*Second Series. By H. WILDE, Esq.**

[With a Plate.]

§ 3. *On the Electric Condition of the Terrestrial Globe, and on the Absolute Character of the Law of Definite Electrolysis.*

86. **I**N a former paper, which I had the honour of communicating to the Royal Society†, reference was made to a certain class of phenomena in which, when presented to the mind for the first time with the view of predicting the magnitude of any effect which would be produced in a body by the addition of proportionate increments of a given force, the results exhibited by experiment are altogether different in magnitude from those which ordinary analogical reasoning would have led to, and consequently produce in the mind the idea of paradox.

87. In that paper several prominent instances of this relation between the quantitative properties of things and the mind of the observer, in new and untried conditions, are also given; and among them perhaps the most interesting are those shown in Tables I. II. III. IV., wherein equal increments of magnetism induce quantities of this same force much greater than those manifested in the original magnet. And, further, a quantity of magnetism or of electricity indefinitely small is demonstrated to be capable of inducing, or making manifest, quantities of these same forces indefinitely great.

88. Another class of phenomena, no less interesting than the one above mentioned, is that in which, by the continued addition of

* Communicated by the Author.

† Philosophical Transactions, 1867, p. 89. Philosophical Magazine, August 1867.

equal increments of a given force to a body, the effects produced are entirely the reverse of those which, without experiment, would have been predicted; and it is only by the employment of a peculiar order of analogies, drawn from a wide range of physical phenomena, that the appearance of these effects in any new condition of things can even be suspected. A remarkable instance of the principle of inverted action here alluded to is that in which a thin plate or disk of metal, when presented to a jet of air escaping under strong pressure from an orifice in a plane surface, is attracted to, instead of being repelled from, the flat surface from which the air issues*. The continued addition of equal increments of heat to a body likewise furnishes many examples of these inversions; but it will be sufficient to mention the spheroidal condition of water in relation to evaporation to indicate the bearing which these remarkable facts have upon the phenomena to be hereafter described.

89. A consideration of many such examples of inverted action as those above referred to led me to suspect that similar actions might exhibit themselves in those departments of electrical science which have hitherto only been explored with apparatus of a limited range of power. In this expectation I have not been disappointed; for, with the powerful electromotors which I have had the good fortune to command, I have not only succeeded in clearing up several points of doubtful knowledge respecting the electric condition of the earth, and of electrolytic action generally, but I have also discovered some new electrochemical facts in connexion with the electrolyzation of water which must ultimately lead to an important modification of our present ideas on the nature of that substance.

90. The experiments which I now purpose to give an account of were made in the course of an inquiry into the possibility of transmitting electric signals through metallic cables submerged without an insulating envelope. This inquiry involved, as a preliminary investigation, a consideration of the function which the earth performs as a conductor of electricity when employed for telegraphic purposes; but before giving an account of this investigation, I will briefly state the opinions respecting the electric condition of the earth as already put forth.

91. At the time when voltaic electricity was first employed for transmitting intelligence to distant places, it was generally believed that a complete metallic circuit, involving the use of two line wires, was absolutely necessary for that purpose, until Steinheil, in 1837, made the discovery that one of these wires might be dispensed with, provided each extremity of the other

* Manchester Memoirs, new series, vol. v. p. 208. Philosophical Magazine, S. 2. vol. v. p. 250.

wire was terminated with a metal plate inserted in the moist ground. This physicist also pointed out that the resistance offered by the earth to the propagation of the current along the wire might be reduced to such an extent as to be quite insensible when compared with that of the double metallic circuit; so that not only was one-half of the wire economized, but even the resistance that such a mixed circuit of earth and wire presented was diminished by one-half.

92. The explanation of this singular and important fact, as given by Steinheil, was that the conducting-power of the earth, though very inferior when compared with that of the metals, yet its bad conductivity was more than compensated for by the immensity of its section. This interpretation of the earth's action naturally led to the opinion that the electric current, after traversing the whole length of the telegraph-wire, returned through the earth back to its origin at the battery, in the same manner as if the chain of earthy particles extending between the distant electrodes had been confined in an insulated tube of large dimensions. The difficulties, however, which encumbered this hypothesis when the distance between the electrodes became considerable, have led many physicists to abandon it as being inconsistent with certain well-ascertained facts.

93. The present view of the earth's action in relation to telegraphic circuits, as supported by Gauss*, De la Rive†, Walker‡, and many others, is, not that the earth conducts the electricity from the distant end of the wire back to its origin at the battery, but that the earth acts as a vast reservoir of neutral electricity, into which the positive and negative currents from each of the insulated wires are absorbed, in the same manner as a discharge of static electricity is received into the ground.

94. In the course of an elaborate series of experiments on the propagation of electric currents in the earth, M. Matteucci made the curious observation that, when the distance between the electrodes at each extremity of the insulated wire was very small, the resistance of the terrestrial bed increased, up to a certain point, in proportion to its length, according to the recognized law for ordinary conductors; but when the distance between the electrodes exceeded thirty yards, the resistance of the earth gradually diminished until at a distance of a few miles it finally became null§.

95. The only explanation which I have as yet met with of this singular limitation of the earth's resistance is that of De la Rive,

* *Traité de Télégraphie Électrique*, by M. l'Abbé Moigno: Paris, 1852, chap. 3.

† Treatise on Electricity, by A. De la Rive, vol. iii. p. 446.

‡ C. V. Walker's 'Electric Telegraph Manipulation,' p. 37.

§ *Annales de Chimie et de Physique*, 1851, vol. xxxii. p. 248.

who thinks that the anomalous results obtained by Matteucci may be accounted for by supposing that the molecules interposed between the two electrodes, not being so numerous when the latter are near together, may constitute the electric chain by the mutual neutralization of their opposite electricities, which is preceded by their polarization; but when the electrodes are very distant from each other this communication can no longer take place, and they are then discharged, by means of the bed with which they are in contact, into the entire mass of the terrestrial globe*. From this explanation it will be seen that De la Rive, in common with Matteucci, holds the opinion that a diminution of the distance between the electrodes in contact with the earth is attended with an increase of resistance to the passage of the current from the electromotor.

96. This brief historical sketch of the present state of knowledge on the function which the earth performs as a conductor of electricity, as made known by those physicists who have preceded me in this research, will, I think, lead to a full comprehension of my own experiments in the same direction, and of the new phenomena which form the subject of this paper.

97. The principal electromotors which I employed were the machines described in the First Series of these researches, and consisted (1) of a $2\frac{1}{2}$ -inch magneto-electric machine (58) worked by hand. This machine would melt four inches of iron wire $\cdot 025$ of an inch in diameter in short circuit, *i. e.* when the iron wire was directly interposed between the polar terminals of the machine.

98. (2) A 5-inch electromagnetic machine excited by a $2\frac{1}{2}$ -inch magneto-electric machine (63).—This machine was furnished with two armatures, one for the production of "quantity," and the other for the production of "intensity" effects. The quantity-armature would melt fifteen inches of iron wire $\cdot 075$ of an inch in diameter, and the intensity-armature thirty-six inches of iron wire $\cdot 035$ of an inch in diameter, in short circuit.

99. (3) A 10-inch electromagnetic machine (66) excited by a $3\frac{1}{2}$ -inch magneto-electric machine.—This machine was provided with the quantity- and intensity-armatures before described (70), (71), (72). The quantity-armature of this machine, when excited by the $3\frac{1}{2}$ -inch machine, would melt about fourteen inches of iron rod $\frac{1}{4}$ of an inch in diameter, and the intensity-armature seven feet of iron wire $\frac{1}{16}$ of an inch in diameter.

100. (4) A well-excited Grove's cell, the platinum of which was $6\frac{1}{2} \times 3$ inches.—This electromotor would melt (making allowance for the heat abstracted by the polar conductors) seven-eighths of an inch of iron wire $\cdot 035$ of an inch in diameter. Two, three,

* Treatise on Electricity, by Ang. De la Rive, vol. iii. note to p. 448.

and four of the same cells, in series, would melt two, three, and four times the length of wire of the same diameter that one cell would melt. Two, three, and four of the same cells, arranged for quantity, would melt a 2-, 3-, and 4-fold strand of the same wire and of the same length that one cell would melt.

101. I have chosen to adopt the melting of a given quantity of wire as the measure of the magnitude of the current from the electromotors above mentioned, because I have not yet found any other method which can at all compare with it in expressing easily and without ambiguity the associated properties of quantity and intensity, especially when dealing with currents of such magnitude as those evolved from the 5- and 10-inch machines. By comparing the absolute quantity of wire melted by a standard Grove's cell with that melted by the machines, under the different conditions to which attention will be directed, a tolerably good idea of the magnitude of the forces brought into operation may be obtained.

102. The other electromotors employed in these experiments were:—(5) a weakly charged battery of thirty-three Daniell's cells, such as are commonly used by the Electric Telegraph Company for signalling-purposes. This battery was arranged in three troughs of eleven cells each. (6) A large well-excited Daniell's cell; and, lastly, a single pair of zinc and platinum plates excited by dilute sulphuric acid, spec. grav. 1.110.

103. The building which contained the various electromotors is situated about 100 feet from the edge of the basin of a navigable canal, in which most of the following experiments on the electric condition of the earth have been made. Connexion between the various electromotors and the electrodes and other conductors submerged in the canal was made by means of two lengths of copper-wire rope, each 140 feet in length and nearly half an inch in diameter. These wire-rope connexions consisted of seven thick copper wires twisted together, and were supported through the air and insulated from one another by means of a pole fixed upon the bank of the canal.

104. The conductors to be submerged, which also served the purpose of electrodes, consisted of two lengths of naked copper-wire rope, each 169 feet long, and of exactly the same diameter and construction as those used for the aerial connexions (103).

105. Two other lengths of copper rope, of the same length and diameter as the preceding (104), were each carefully wrapped with a double covering of linen tape, in the manner adopted for insulating the coils of the large electromagnet of the 10-inch machine (68). These two lengths of tape-covered copper rope were laid parallel with each other and then bound tightly together with string wound round them in a spiral direction, so

that though the conductors were in close proximity to each other, yet no metallic contact between them was established. The integrity of the insulation of these double or twin conductors (as I propose to call them) was examined in air with the powerful current from the 10-inch intensity-armature, and was found to be perfect. These double or twin conductors are represented in fig. 11, Plate I., with the ends A, B and C, D carefully separated.

106. Owing to the great size and good conducting-capacity of the aerial connexions, and of the naked and the tape-covered conductors to be immersed in the canal, the whole of the 618 ($2 \times 140 + 2 \times 169 = 618$) feet of copper rope, when formed into a complete metallic circuit in air, did not greatly diminish the power of the current from any of the electromotors, except that from the 10-inch quantity-armature, which current, however, after passing through the 618 feet of conductor spread out on a dry boarded floor, was still sufficiently powerful to melt twelve inches of iron wire $\cdot 095$ of an inch in diameter when introduced into the circuit; while the currents from the 5- and 10-inch intensity-armatures, after passing through the same length of conductor, would melt nearly the same quantity of wire as they would in short circuit; and the currents from the $2\frac{1}{2}$ -inch machine and the single Grove's cell, after passing through the 618 feet of wire rope, would still melt, the former 4 inches, and the latter $\frac{3}{4}$ of an inch of iron wire $\cdot 016$ of an inch in diameter.

107. As this size of iron wire ($\cdot 016$ of an inch in diameter) will be frequently referred to hereafter, it will be more convenient to speak of it as the thin wire.

108. The two naked copper ropes (104) were each coiled into a ring about 2 feet in diameter, with their several convolutions in close metallic contact. The rings were then placed in metallic connexion with the respective ends of the aerial conductors and immersed in the canal, with the planes of the rings opposite to each other, about three feet apart, and perpendicular to the surface of the water.

109. The disposition of the aerial conductors and electrodes in relation to each other and to the electromotors will be better understood on reference to Plate I. fig. 12, in which E represents the electromotor, F, G its polar terminals, H, I the aerial conductors for establishing connexion between the electromotor and the ring-electrodes K, L immersed in the canal M. The respective ends of the naked copper ropes will be indicated, when necessary, by the same letters (A, B and C, D) as those of the twin conductors, fig. 11.

110. A tangent galvanometer, or pieces of iron wire of different lengths and diameters, could be introduced at pleasure at the point N, between the pole G and the conductor H, for the purpose of indicating, by galvanometric deflection, or by the heat

produced in the iron wires, the passage of a current through the conductors into the earth.

111. The current from the battery of thirty-three Daniell's cells in series was first transmitted through the tangent galvanometer in short circuit, when the needle stood permanently at 10° .

112. When the same battery-current was transmitted into the canal, through the conductors and electrodes arranged as in fig. 12, the galvanometer introduced at N still indicated 10° of current, thereby showing a *null resistance* on the part of the water in the canal to the passage of the whole amount of current which the battery was capable of producing.

113. But on attempting to transmit the powerful alternating currents from either the quantity- or the intensity-armatures of the 5- and 10-inch machines into the canal through the same arrangement of conductors and electrodes as in the preceding experiment, the amount of current actually transmitted was not sufficient to make three inches of the thin iron wire (107) introduced at N sensibly warm to the fingers, the result now indicating an almost *total resistance* to the passage of the current which the electromagnetic machines were capable of producing.

114. On uncoiling the two rings of copper rope and extending them in straight lines, about twenty feet apart, along the bottom of the canal towards the points O, P, with the distant ends C, D of the conductors entirely disconnected, the current from the 5-inch intensity-armature made two feet of iron wire '035 of an inch in diameter bright red-hot at N, and the current from the 10-inch intensity-armature melted four feet of wire '050 of an inch in diameter, the result now indicating a great diminution of the apparent resistance of the earth to the passage of a powerful electric current.

115. But on attempting to transmit the current from the 5- and 10-inch quantity-armatures through the conductors extended in the canal, the amount of current now transmitted was only sufficient to make fifteen inches of the thinner wire used in the preceding experiment (114) sensibly warm to the fingers, the result again exhibiting an increase of the earth's resistance similar to that observed when the conductors were coiled up into rings.

116. Similar effects of resistance to the passage of the current into the earth, as indicated by the non-heating of the thin wire at N, were also obtained when the $2\frac{1}{2}$ -inch machine, or the single Grove's cell, was connected with the submerged conductors in their extended form.

117. That this resistance to the passage of the current from the 5- and 10-inch quantity-armatures and that from the single Grove's cell was not due to the resistance of the copper conduc-

tors themselves (106), or to any fault in the connexions, was at once made evident by joining together the distant ends C, D of the extended conductors, when the iron wires introduced at N were immediately melted.

118. In order that a discharge of electricity through the extended conductors K, L might be obtained, it was essential that the connexion between the ends A, B and the aerial connexions H, I, although immersed in the water, should be entirely metallic; for when one of these ends, A, was separated from the other end, I, by only the thickness of a piece of tape, no heating effects sensible to the touch were produced in the thin iron wire at N by the powerful current of the 10-inch intensity-armature, even when the distant extremities C, D of the conductors were joined together.

119. Though the current from a single Grove's or Daniell's cell was not powerful enough to heat sensibly any thin wire at N when either of these cells was placed in connexion with the extended conductors with their ends C, D disconnected, yet an amount of current was always found passing through the conductors sufficient to produce a deflection of the galvanometer at N. Moreover, when contact between the Grove's or Daniell's cell (or one formed of zinc and platinum, excited by dilute sulphuric acid) and the extended conductors was maintained for a few moments and then broken, the galvanometer always indicated a counter current from the extended conductors about half the strength of that previously passing from the electromotor.

120. That none of the effects of conduction and resistance of the terrestrial bed, as manifested by the heating or non-heating of the wires at N, were brought about by the exercise of any mutual influence of the conductors upon each other through the intervening body of water, or, again, by any change in the electric condition of the earth taking place by reason of a change in the linear distance of the electrodes from each other, was proved by the following experiments (122, 123) (137-139).

121. One of the aerial copper-rope conductors, H, was extended upon insulated supports along the side of the canal to a point 300 feet beyond where the other aerial conductor, I, entered the canal, so that the electrodes or conductors in the canal might now be 300 feet instead of a few feet from each other. In consequence of the great thickness and good conducting-capacity of the aerial connexions, they offered, practically, no resistance to the passage of the currents from any of the electromotors used in the experiments.

122. On repeating, in the same order, all the preceding experiments with the submerged conductors, both in their coiled and extended form, results were obtained from the electromotors

identical in every respect with those observed when the electrodes were only a few feet apart. Thus a series of thirty-three Daniell's cells indicated 10° of current on the galvanometer at N, whether the distance between the ring-electrodes was 3 feet or 300 feet; while with the quantity- and intensity-currents from the 5- and 10-inch electromagnetic machines the opposite effects of resistance, as manifested by the non-heating of thin wire at N, were obtained, whether the ring-electrodes were at the greater or at the less distance (113).

123. Again, when the conductors were uncoiled and extended in the canal 300 feet from each other, the currents from the 5- and 10-inch intensity-armatures melted and made red-hot the same lengths and thicknesses of wires at N as when the conductors were extended in the canal 9 feet apart; while, on the other hand, the currents from the 5- and 10-inch quantity-armatures encountered the same resistance, as manifested by the non-heating of the wires at N, at the greater as at the less distance (115).

124. The following Table contains the results of a carefully made series of experiments with different numbers of similar Daniell's cells, when the ring-electrodes were immersed in the canal at distances of 3 and 300 feet from each other.

TABLE V.

No. of Daniell's cells in series.	Deviation of galvanometer with battery in short circuit.	Deviation of galvanometer when distance between electrodes = 3 feet.	Deviation of galvanometer when distance between electrode = 300 feet.
1	9.5	3.75	3.75
2	10.0	5.75	5.75
5	10.0	7.50	7.50
11	10.0	8.75	8.75
22	10.0	9.75	9.75
33	10.0	10.0	10.0

125. The results contained in columns 3 and 4 of the above Table prove conclusively that no change whatever occurs in the electric condition of the earth in consequence of a mere change in the distance of the electrodes from each other. It will also be evident that variations in the intensity of the battery-current are sufficient to produce corresponding changes in the resistance of the earth similar to those produced by the quantity- and intensity-currents from the electromagnetic machines when the conductors were extended in the canal (114, 115).

126. Returning to a consideration of the effects produced by the alternating current from the 5-inch intensity-armature when the ring-electrodes were connected with the aerial conductors of

equal length (113):—Although, when the electrodes were three feet apart, a sufficient amount of current was always passing into the canal to produce a spark and shock on breaking contact at N, yet it was insufficient, as we have seen, to heat sensibly wire so thin as $\cdot 016$ of an inch in diameter. When, however, the electrodes were made to approach so as to be within one inch of touching each other, a length of three inches of the above-mentioned wire was made red-hot.

127. In order that this mutual influence which the electrodes exercised on each other, to promote an increase of discharge from the electromotors, might be more closely investigated when the distance between the electrodes was still further reduced, the tape-covered twin conductors were submerged in the canal in their extended form, as shown in fig. 11, with the ends A B connected with the respective ends of the aerial conductors, and the distant ends C, D entirely disconnected. From the porous nature of the tape-covering with which the conductors were enveloped, it will be obvious that this covering, though a good insulator in air, would, when saturated with water, offer little or no obstruction to the passage of an electric current either into the earth or between the conductors.

128. When the current from the 5-inch intensity-armature was transmitted through the submerged twin conductors, 2 feet of the same-sized wire which was made red-hot at N when the naked conductors were extended in the canal (114) was now melted. And the current from the 5-inch quantity-armature, which previously only made 15 inches of wire $\cdot 021$ of an inch in diameter warm to the touch, now melted this same wire, and also made red-hot 15 inches of thicker wire, $\cdot 050$ of an inch in diameter.

129. On coiling the twin conductors into a double ring about 2 feet in diameter and immersing it in the canal in the place previously occupied by the separate ring-electrodes of naked copper rope, the 15 inches of thicker wire ($\cdot 050$ of an inch in diameter), which was only made red-hot by the 5-inch quantity-armature in the last experiment, was now melted, thereby indicating the passage of a more powerful current from the electromotor when the twin conductors were coiled into a ring than when they were extended along the bed of the canal. With this arrangement of the double coil, the current from the 10-inch intensity-armature melted 6 feet of wire $\cdot 065$ of an inch in diameter.

130. That the increased quantity of current passing through the twin conductors, above that transmitted through the naked conductors, whether in their coiled or extended form, was due to the direct influence which the twin conductors exercised on

each other, and is an effect entirely distinct from that which the earth produces when receiving into itself a discharge of electricity, was at once made evident by lifting the saturated double coil out of the canal and suspending it in the air, when the current from the 5-inch quantity-armature was still sufficiently powerful to make bright red-hot, at N, 15 inches of the same wire which was only made warm to the touch when the naked conductors were extended separately in the canal (115).

131. That the resistance which the naked ring-electrodes presented to the passage of the current from the 5-inch and 10-inch intensity-armatures did not arise from the circumstance of their convolutions being in close metallic contact with each other, was proved by the fact that the same resistance to the passage of the current from these armatures (as shown by the non-heating of thin wire at N) was experienced when the tape-covered conductors were coiled up into two separate rings and immersed in the canal at the same distance from each other as were the naked ring-electrodes.

132. Suspecting that the resistance which the two tape-covered coils when separated offered to the transmission of the current into the earth was due to an inductive action set up among the several convolutions of each coil and operating to produce a repulsion of the current on itself, the following experiments were made for the purpose of testing the accuracy of this view.

133. Each length of the tape-covered conductors was, in the first place, uncoiled, and extended in a straight line along the bottom of the canal, with a lateral distance between them of about 20 feet. On transmitting the current from the 5-inch intensity-armature through the conductors, a length of 2 feet of iron wire .035 of an inch in diameter was made bright red-hot at N, showing the passage of the same amount of current as when the naked conductors were extended in the canal under like circumstances.

134. Each of the conductors was then bent backwards and forwards along the bottom of the canal in three lengths of 56 feet, so that the conductors were each of them in the form of an elongated letter N. On transmitting the current from the same armature through this arrangement of conductors, the 2 feet of wire at N was made somewhat less bright red than in the previous experiment.

135. Again, on binding the three lengths of each of the conductors together with string, and immersing each of the conductors thus folded in the canal, as shown in fig. 13, the 2 feet of wire at N, though hotter than could be endured by the touch, was not made visibly red-hot.

136. From a consideration of the opposite effects of resistance and conduction exhibited when the tape-covered conductors were arranged separately in the canal in the several ways described (131, 133, 134, 135), it is evident that the transmission of the current from the electromotor is preceded by an inductive action upon the liquid particles in contact with the conductors, which action, according as it is adverse or favourable, retards or accelerates the discharge of the current into the earth. The increased quantity of electricity discharged into the earth from the three-fold conductors, above that from the same conductors when coiled into separate rings, is not surprising, as we have already seen that in the case of the coiled twin conductors the inductive action of a number of convolutions in contact with each other on every side was much more perfect (as shown by the melting of thick wires at N) than when these same twin conductors were extended in the canal (128, 129).

137. To recapitulate what has already been advanced in proof of the action of the electrodes being independent of any mutual influence which they might be supposed to exercise on each other through the intervening body of water when transmitting a current of electricity into the earth:—We have seen that the same effects of conduction and resistance were produced, and equal amounts of current were transmitted into the earth from the same electrodes, whether the distance between them was 3 feet or 300 feet—a result altogether incompatible with the well-ascertained laws which have been determined in connexion with the transmission of electric currents through liquids contained in insulated vessels.

138. That the distance between the electrodes does, under certain circumstances, exercise an influence on the quantity of electricity transmitted from the electromotors was evident from the heating and fusing of wires at N when the conductors were brought into close proximity to each other by the contrivance of the twin conductors. But that this influence operates within limits extremely small was also evident from the fact that, when the ring-electrodes were only 3 feet apart, none of the electromotors would heat the thin wire at N. Moreover, that the mutual influence of the electrodes upon each other when in close proximity is an action entirely distinct and separable from the influence which these electrodes exercise upon the earth is shown by the heating of wires at N when the saturated coil of twin conductors was lifted out of the canal and suspended in the air (130).

139. This distinction between the two inductions becomes still more obvious from the fact that, when the twin conductors were extended in the canal, the inductive influence of the con-

ductors upon each other acted in *conjunction* with the normal inductive action of the earth to promote *an increase* of discharge from the electromotors above that observed when the naked or the tape-covered conductors were extended separately in the canal (114, 115, 128). On the other hand, when the tape-covered threefold conductors were extended separately in the canal (fig. 13), the inductive influence of each conductor upon itself acted *in opposition* to the normal action of the earth, so as *to diminish* the amount of discharge from the electromotor below what was observed when the same conductors were extended in straight lines, or arranged in the form of the letter N.

140. Reverting to the fact that when contact was made between the 5- and 10-inch intensity-armatures and the conductors extended separately in the canal, the heating and melting of wires at N indicated the passage of a powerful current into the earth (114, 123):—A question of great importance now arose as to whether this current was transmitted in consequence of a physical conductivity of the terrestrial bed, or by the electrolyzation of the liquid in contact with the conductors. Hitherto electricians have been content to affirm that the earth absorbs or drinks up the electricity transmitted from the electromotors. Matteucci had indeed found that when the electrodes were plunged into the soil impregnated with sulphate of copper or sea-water, the quantity of electricity discharged from the electromotor was increased; but, beyond this fact, no experiments have yet been recorded with the view of establishing the absolute dependence of the transmission of the current into the earth upon the electrochemical properties of the liquids incorporated with it.

141. One important step towards the establishment of this dependence was the diminution of the earth's resistance observed when the tension of the current transmitted along the conductors was increased (114, 115)—a result entirely in accordance with what is observed in the electrolyzation of water in insulated vessels. Moreover we have also seen that though the discharge of the current into the earth from the extended naked conductors was distinct from, as well as less in amount than, the discharge between the twin conductors extended in the same liquid, yet it was of the same kind and in the same direction in the one case as in the other. The difference, then, between the discharge of the conductors into the earth and into each other being one of degree only, it seemed to me that some advantage might be derived from the study of the action of the naked and tape-covered conductors upon each other when coiled and immersed

in insulated vessels, for the purpose of comparing the results with those obtained when the same conductors were immersed in liquids in contact with the earth.

142. A large wooden tub, partially filled with the same canal-water as that in which the previous experiments were made, was insulated from the ground by means of three fireclay bricks (fig. 14); and in this tub the two ring-electrodes of naked copper rope were immersed with the planes of the rings about 3 inches apart.

143. On making contact between the 5-inch intensity-armature and the coils immersed in the tub, a resistance to the passage of the current (as manifested by the non-heating of thin wire at N) was experienced, similar to that which was observed when the same electrodes were immersed in the canal (113). On diminishing the distance between the electrodes from 3 inches to 1 inch, the 3 inches of thin wire .016 of an inch in diameter was now made red-hot, as was the case when these coils were immersed at the same distance from each other in the canal (126).

144. Similar effects of resistance to the passage of the current from the 5-inch intensity-armature to those obtained with the naked ring-electrodes were also experienced when the tape-covered ring-electrodes were immersed separately in the tub at the same distances from each other as in the preceding experiment.

145. Again, no transmission of the current sufficient to heat thin wire at N occurred when contact was made between the coiled electrodes, when 3 inches apart, and the $2\frac{1}{2}$ -inch machine, or a single Grove's cell—the results agreeing in this respect also with those obtained in the canal.

146. The tape-covered conductors were now rearranged in the tub as a double coil of twin conductors, bound together with string, with the ends C, D disconnected.

147. On transmitting the current from the 10-inch intensity-armature through the immersed coil, 5 feet of wire .065 of an inch in diameter was melted at N, while the current from the 5-inch quantity-armature made 15 inches of wire .050 of an inch in diameter bright red-hot, whereas the current from the same armatures melted respectively 6 feet and 15 inches of the same-sized wires when the double coil was immersed in the canal (129)—a result which again shows, but in a converse manner, that the mutual influence of the conductors upon one another was supplemented by the inductive influence of the earth, as in the case where the conductors were extended jointly and separately in the canal (114, 128).

148. Contact was now made between the double coil and a single Grove's cell; but although the galvanometer indicated

the passage of a tolerably strong current through the coil, yet it was not sufficiently powerful to make the thin wire red-hot at N.

149. When contact was made between the double coil and the single pair of zinc and platinum plates contained in a glass vessel (102), the passage of a current through the water in the tub was indicated by the liberation of bubbles of hydrogen from the platinum plate, as well as by the deflection of the galvanometer at N. This result was also obtained when the current from the same pair of plates was transmitted through a small double coil of twin conductors (165) immersed in a glass of distilled water.

150. When the ends A, B of the double coil in the tub, or of the coil in the glass vessel, were connected with the respective terminals of the galvanometer, after all contact with the voltaic couples had been broken, the galvanometer always indicated a strong secondary current from the coils, similar to that observed when the conductors were extended separately in the canal (119).

151. A quantity of common salt was now dissolved in the tub of fresh water, sufficient to make the solution as nearly as possible of the same specific gravity as sea-water. On repeating the experiments with the naked and tape-covered coils immersed separately in the tub with the planes of the coils 3 inches apart (142), the currents from the 5- and 10-inch intensity-armatures which, when the coils were at the same distance apart in fresh water, would not melt any thin wire at N, now melted 30 inches of iron wire $\cdot 035$ of an inch in diameter, and 6 feet of wire $\cdot 065$ of an inch in diameter respectively; and the current from the $2\frac{1}{2}$ -inch machine now melted 4 inches of the thin wire (107).

152. When the tape-covered conductors were again arranged as a double coil of twin conductors, so little resistance was offered to the transmission of the alternating current through the liquid, that the 5- and 10-inch intensity-armatures now melted the maximum quantity of wire at N that they were capable of melting in short circuit; and the current from a single Grove's cell would now make $\frac{3}{4}$ of an inch of the thin wire red-hot.

153. Instead of the bright flash and combustion of the copper, which accompanied the interruption of the current from the 10-inch intensity-armature on making and breaking contact between the ends C, D of the double coil while dry and suspended in air, a small spark only was visible when it was immersed in salt water, and the thin wire, when introduced between C and D, was not made red-hot.

154. On substituting dilute sulphuric acid, spec. grav. 1.110, for the salt water in the tub, and transmitting the alternating

current from the 10-inch intensity-armature through the coil, so perfect was now the communication between the conductors through the electrolyte, that not the smallest spark could be obtained on making and breaking contact between the ends C and D.

155. A phenomenon already observed, on a small scale, by De la Rive* when engaged in transmitting the alternating currents from a magneto-electric machine through dilute sulphuric acid by means of platinum electrodes, was here reproduced in a most remarkable manner. When the coils were immersed in the tub of fresh or salt water, or in dilute sulphuric acid, it was observed that not a single bubble of gas arose to the surface of the liquids, no matter for how long a time the powerful alternating currents from the 5- and 10-inch intensity-armatures were transmitted.

156. That these currents were capable of producing energetic electrochemical effects, simultaneously with the non-appearance of gas in the tub, was proved by introducing into the circuit at N a pair of large platinum plates plunged in a glass vessel containing dilute sulphuric acid, when an abundant liberation of the mixed gases from each of the plates occurred, while at the same time no gas was liberated from the coils in the tub. Hence it seemed as though the alternating current could be transmitted through the same kind of liquid, either with or without electrolyzation, by simply varying the arrangement of the electrodes.

157. When, however, the direct current from any of the armatures was transmitted through the double coil, the passage of the current was in all cases accompanied by the liberation of hydrogen from the liquid in which the coil was immersed.

158. That the absence of any appearance of electrolyzation of water in the tub, when the alternating currents of the 5- and 10-inch intensity-armatures were transmitted through the coils, was due to the recombination of the electrolytic products formed by each alternation of the current, was made evident in the following manner.

159. The double coil of twin conductors was immersed in the tub of salt water, and the ends C, D (fig. 14) were connected by means of a short piece of the thin iron wire. One of the other ends, A, was connected directly with the zinc of the single Grove's cell, while contact with the other end, B, and the platinum was established at N by means of another short piece of iron wire somewhat thicker than that which joined the ends C, D.

160. When the current from the Grove's cell was transmitted through the coil, the thin wire at C D, after a few moments, gra-

* *Comptes Rendus*, 1837, vol. iv. p. 837.

dually became red-hot, while the thicker wire at N, though hotter than could be endured by the finger, was not made visibly red during the maintenance of the battery-contact.

161. On reversing the battery-connexions, by joining the end A to the platinum and the end B to the zinc by means of the thicker wire at N, this wire immediately became bright red-hot, while the thinner wire at C D remained cold; but on continuing the battery-contact for about a minute, the thicker wire at N gradually became less bright, until it finally ceased to be visibly red, when the thin wire at C D again became red-hot; and as often as the battery-connexions were reversed, so often did the wires at the battery and distant ends of the coils become alternately, but never simultaneously red-hot.

162. Another phenomenon attending the alternate heating of the iron wires was the liberation of hydrogen from the double coil only during such time as the thin wire at C D was maintained at a red heat, but never while the thicker wire at N was heated; for as often as the battery-connexions were reversed, the liberation of hydrogen from the coil immediately ceased, and did not reappear until the thin wire at C D was reheated.

163. Moreover, on making and breaking contact between the ends A, B of the double coil after they had been maintained in connexion with the Grove's cell for a short time with the ends C, D disconnected, sparks could be obtained from the coil for more than five minutes after all connexion with the electromotor had been broken; the galvanometer, as might have been expected, always indicated a strong secondary current from the coils, in the reverse direction to that transmitted by the electromotor.

164. From a consideration of the foregoing experiment, made with the view of explaining the absence of any appearance of electrolyzation of the water in the tub by the alternating currents from the electromagnetic machines, it will be evident, from the heating of the thin wire at C D when contact was first made between the battery and the double coil, that the hydrogen evolved by the electrolyzation of the water formed an insulating envelope surrounding one of the conductors, which thereby enabled a considerable portion of the current from the electromotor to traverse the conductors instead of being transmitted through the water. But when the battery-contacts were reversed, the cessation of the stream of hydrogen from the coil, and the non-heating of the thin wire at C D, indicated the reunion of the hydrogen envelope with the liquid in contact with the conductor; while the hydrogen evolved by the new current on the other conductor did not become visible until its affinity for the oxide produced by the previous current had been satisfied. But by continuing the battery-contact, a new hydrogen

envelope was formed, which enabled the comparatively feeble current from a single Grove's cell to exhibit, by its prolonged action, electrodynamic effects at C D which the powerful, but momentary, alternating currents from the 10-inch intensity-armature were unable to produce.

165. Proceeding from the liquids in the tub which offered the least to those which presented the greatest amount of resistance to the passage of an electric current, a small double coil of twin conductors was immersed in a large glass jar of Gallipoli oil with the ends C, D disconnected. This coil was formed of two separate lengths of cotton-covered copper wire, each 150 feet long and .050 of an inch in diameter.

166. On making and breaking contact between the ends A, B of the double coil and the polar terminals of the 10-inch intensity-armature while the coil was immersed in the jar, not the slightest spark, nor any other indication of the passage of a current through the oil, was obtained—the result being, in this respect, the same as if the ring of twin conductors had been suspended in the air.

167. That the peculiar arrangement of the double coil was eminently favourable for exhibiting the transmission of a current through a conducting liquid was evident from the fact that when the same coil was immersed in the jar filled with dilute sulphuric acid instead of oil, so powerful was the current transmitted through the liquid, that a length of 2 feet of each of the ends A, B, extending from the surface of the liquid to the terminals of the electromotor, was made red-hot. This experiment, besides exhibiting the greatest possible difference between the conducting-power of liquids under conditions eminently favourable for obtaining an electrolytic discharge, also shows with what enormous rapidity the liberation and recombination of the electrolytic products takes place; for during the whole time that the ends A, B of the copper wires were maintained at a red heat, not a single bubble of gas made its appearance in the dilute acid in which the double coil was immersed.

168. I have said that when an electromotor consisting of a single pair of zinc and platinum plates, excited by dilute sulphuric acid, was placed in connexion with a double coil of twin conductors immersed in a vessel of distilled water, the transmission of the current through the water was attended by the liberation of hydrogen from the platinum in a visible manner (149).

169. As this result has never, to my knowledge, been obtained before by a current of so low an intensity as that from a single pair of zinc and platinum plates, it seemed to me that the anomalous exception which Faraday thought he had discovered to his

own law of definite electrolysis, when experimenting with a similar pair of plates, had no real existence. Nevertheless, as exception might probably be taken to the fact that the coil was made of an oxidizable metal (though I have no doubt that a coil of platinum wires would behave in precisely the same manner), and in order that a question that has excited so much discussion amongst physicists, and is still regarded by some as an undetermined point in electrical science, might be definitely settled, I was induced to repeat Faraday's experiment as nearly as possible in the manner in which he himself conducted it*, but with certain variations which suggested themselves to me while observing the effects produced by the alternating current from the single Grove's cell upon the double coil when immersed in the tub of salt water.

170. Two glass vessels, Q and R, fig. 15, were filled with the same dilute sulphuric acid, spec. grav. 1.25. The vessel Q contained a single pair of amalgamated zinc and platinum plates, which were connected respectively, by means of thick copper wires, with two platinum plates immersed in the vessel R. On introducing a very sensitive galvanometer, or a bit of bibulous paper saturated with a solution of iodide of potassium, into the circuit of one of the connecting wires, the deflection of the needle or the evolution of iodine indicated the passage of an electric current through the liquids in both vessels; but no matter for what length of time connexion was maintained between the electromotor Q and the electrodes in R (by means of the thick wires), no liberation of gas appeared on the platinum plates in either of the vessels. Hence it was that Faraday inferred that a feeble current might be transmitted from an electromotor without the electrolyzation of the liquids in contact with the electrodes. So convinced, however, was I that the absence of any appearance of electrolyzation in this case was due to the resistance and reactions of the nascent gases enveloping the platinum plates in Q and R, that it only appeared necessary for me to alternately invert the direct current from the electromotor Q, before it arrived at the electrodes in R, in order that the same recombination of gas on the electrodes and increased discharge from the electromotor might be brought about as when the alternating current from the Grove's cell was transmitted through the coil immersed in the tub of salt water.

171. This alternation of the direction of the current was effected by means of a commutator, S, fig. 15, placed between the vessels Q and R. Care was taken that the alternating metals of the commutator were separated from each other by a sufficient thickness of insulating material to prevent the metals which re-

* Philosophical Transactions, vol. cxxiv. p. 448.

ceived the inverted current from bridging across or making a short metallic circuit between them and the plates in the vessel Q, as, otherwise, deceptive results would have been obtained.

172. When the commutator was at rest, but arranged so that the zinc and platinum plates in Q should be in metallic communication with the electrodes in R, no liberation of gas appeared on the platinum plates in either of the vessels. But on making the commutator revolve at such a velocity that the current, after leaving Q and before arriving at R, should be alternately inverted about 1000 times per minute, the whole surface of the platinum plate in Q gradually became covered with bubbles of hydrogen, which finally detached themselves from the plate and escaped through the liquid in the same manner as when the same pair of zinc and platinum plates were connected (without the intervention of a commutator) with the double coil of wire immersed in distilled water (149).

173. The proof that the passage of an electric current through the vessels Q and R was accompanied by the electrolyzation of the liquid in Q was therefore full and complete. Now Faraday has shown (*Experimental Researches*, 809, 991) that when several electrolytic cells, containing the same dilute sulphuric acid, are arranged in the same circuit, the amount of electrolytic action is absolutely equal in each cell; consequently whatever be the amount of electrolytic action in the vessel Q, an equal amount of the same kind of action, whether visible or invisible, must inevitably take place in the vessel R. It is therefore demonstrated, both by experiment and by the law of simultaneous definite action, that the transmission of an electric current through an electrolyte is invariably attended by the electrolyzation of those sections of it which are in contact with the electrodes; and, consequently, without electrolyzation the passage of an electric current from an electrode into an electrolyte does not occur (166).

174. Before dismissing this part of my researches on the transmission of electric currents through liquids contained in insulated vessels, which have been made with the view of comparing the results with those obtained with similar liquids when forming part of the terrestrial bed, I must not omit to make reference to the important discoveries of Ritter, Faraday, Grove, De la Rive, and others, on the influence which metallic surfaces exercise in producing gaseous combination, and of the secondary currents attending such combination, all of which are connected more or less intimately with the experiments which I have described. To De la Rive especially belongs the merit of effecting the transmission of a current through a liquid without any appearance of electrolyzation, by employing the current from a voltaic battery of considerable intensity, alternated by means of

a commutator, instead of that from a magneto-electric machine. He also found that an alternating current meets with less resistance when transmitted through an electrolyte than does the direct current from the same voltaic battery, and rightly conjectured that the non-appearance of the gases liberated by the alternating current was due to their recombination on the surface of the electrodes. Moreover De la Rive, in common with many other physicists, has maintained that the transmission of the current, and the electrolyzation of the liquid in contact with the electrodes, are inseparably connected. I do not, however, find that any one has yet given a direct proof, or one free from ambiguity, that the transmission of a current of so low an intensity as that used in Faraday's experiment is dependent upon the electrolyzation of the liquid through which such current is transmitted. Hence it is that up to the present time the absolute generality of the law of definite electrolysis has been doubted by some philosophers, and has never acquired more than a high degree of probability with others.

175. Returning to the phenomena exhibited by the conductors when in contact with the terrestrial bed:—It has been shown that all the effects of resistance and conduction produced by the naked and tape-covered coils immersed in the tub of fresh water were identical with those exhibited when these same coils were immersed in the canal (113, 143), (131, 144), (129, 147), (119, 150). We have also seen that the current from the $2\frac{1}{2}$ -inch machine, which would not heat thin wire when the conductors were coiled up separately in the canal or in the tub of fresh water, would nevertheless melt four inches of this wire at N when the coils were immersed in the tub of salt water at a distance of 3 inches apart (151). Hence it seemed highly probable that similar differences of resistance to the passage of the current would also be found between the fresh and salt water in contact with the terrestrial bed.

176. Two of the copper-rope conductors used in the canal experiments were therefore extended separately in the sea, on the coast of Lancashire, in about one fathom of water. On transmitting the current from the $2\frac{1}{2}$ -inch machine through the extended conductors, the same quantity of thin wire (four inches) was melted at N as when the conductors were coiled up separately in the tub of salt water (151).

177. But when the submerged conductors were placed in connexion with the single Grove's cell, the current which made three-quarters of an inch of thin wire red-hot at N when the double coil was immersed in the tub of salt water (152) was

now insufficient to make the same length of thin wire hotter than could be endured by the finger, the result thereby indicating a greater amount of resistance to the passage of the current into the earth than that which the salt water in the tub presented to the passage of the current between the same conductors when in the form of a double coil. This difference of resistance to the passage of the current, according as the conductors were extended in the sea or coiled up together in the tub of salt water, coincides with the difference which we have already observed when the same conductors were coiled up in the tub of fresh water and extended separately in the canal (115, 147).

178. To sum up the results of these experiments, as far as we have gone, on the transmission of electric currents through liquids contained in insulated vessels, and those forming part of the terrestrial globe:—By operating in both cases with the same electromotors and arrangement of conductors, all the various phenomena of conduction and resistance obtained in the tub of fresh and salt water were identical in kind with those obtained in the canal and in the sea, the difference in degree being due to the close proximity of the twin conductors when in the form of a double coil in the tub, which, as we have seen, augmented the amount of discharge from the electromotors above that which the normal action of the earth induced when these same conductors were extended separately in contact with it. From the increased amount of discharge obtained from the 10-inch intensity-armature when the double coil was immersed in dilute sulphuric acid (154), and the absolute stoppage of the current when a similar double coil was immersed in a vessel of oil (166), we may justly infer that similar extremes of conduction and resistance would manifest themselves if these liquids formed part of the terrestrial globe, so that the earth would consequently become either a conductor or an insulator, according to the nature of the liquids incorporated with its substance. Again, the resistance which the earth, whether forming the bed of the sea or of the canal, presents to the passage of a current of low intensity, and the diminution of this resistance by an increase of the intensity of the current, coincide with the same order of resistances and intensities of the current observed in the electrolyzation of liquids in insulated vessels. We have also seen that the transmission of the current from even the most feeble of the electromotors used in these experiments, either through dilute sulphuric acid or distilled water, was in every case attended by the electrolyzation of the liquids into which the electrodes were plunged. Hence we may justly conclude, even without the exhibition of the electrolytic products themselves (which will, however, be shown in experiments to be hereafter adduced (198–205)), that the transmis-

sion of an electric current from a conductor into the earth is invariably accompanied by the electrolyzation of the liquids in contact with this conductor, and that without such electrolyzation transmission of an electric current from an electrode into the earth does not occur.

179. For this reason I have not thought it necessary to make any experiments upon the electric condition of the earth by inserting electrodes in the ground at different distances from each other, as, owing to the varying degrees of moisture on different parts of the earth's surface, arising from differences of elevation, of climate, and of geological structure, it would be impossible by such means to arrive at any general law on the subject. Moreover, as the normal inductive action of the earth at any given point is, as we have seen, dependent upon the facility with which the liquids in contact with it can be electrolyzed, it follows that the electrolyzation of any liquid artificially introduced into the earth at that point cannot truly represent the amount of discharge which the earth is capable of inducing from a given electromotor (116, 176). We can easily understand, therefore, how Matteucci, by establishing communication between his electrodes and the earth by means of a solution of sulphate of copper or other liquids with which he impregnated the ground, and also by a variable degree of saturation of the soil, might obtain those anomalous variations of the earth's resistance which he attributed to differences in the length of the terrestrial matter extending between the electrodes, but which anomalous variations I have not met with in the course of my investigations.

180. Hitherto all the phenomena which we have observed to attend the passage of an electric current into the earth have been manifested by the heating of wires and by other effects produced at the electromotor ends of the submerged conductors, while the distant ends of the latter were disconnected. I will now proceed to describe some of the effects produced at the distant ends C, D of the conductors when extended separately in the canal and in the sea.

181. The two naked copper conductors (104) were extended along the bottom of the canal at a sufficient distance from each other to prevent any lateral discharge from taking place between them. A length of about 3 feet of each of the distant ends C, D of the conductors was allowed to project out of the water for the purpose of introducing iron wires or a tangent galvanometer into the circuit. The other ends, A, B, were joined on to the respective polar terminals of the electromotors.

182. When the current from a single Daniell's cell, or that

from a pair of zinc and platinum plates excited by dilute sulphuric acid, was transmitted through the submerged conductors, the galvanometer at C, D indicated the same degree of current as when these conductors were extended in the air, thereby showing that the current had sustained no appreciable loss in consequence of the non-insulation of the conductors.

183. On transmitting the current from the single Grove's cell through the submerged conductors, $\frac{5}{8}$ of an inch of the thin wire was melted at the distant point C, D, being nearly the same quantity as was melted by the same cell when the conductors were extended in the air (106).

184. On transmitting the current from the $2\frac{1}{2}$ -inch machine through the submerged conductors, the quantity of thin wire melted at C, D was 3 inches, whereas 4 inches of the same wire was melted when the conductors were extended in the air,—the result indicating, as in the experiment with the single Grove's cell, that an increase in the electric intensity was attended by a diminution of the amount of current which arrived at the distant ends of the conductors.

185. This diminution of the amount of an intensity-current arriving at the point C, D, reached its maximum when the current from the 10-inch intensity-armature was transmitted through the submerged conductors, as in this case the current arriving at C, D, though very considerable, was only sufficient to melt 4 feet of wire .050 of an inch in diameter instead of 6 feet of wire .065 of an inch in diameter, which the machine would melt when the conductors were in air. In this experiment it will be seen that the quantity of wire melted at C, D is the same as that melted at N by the current from the same armature when the ends C, D were disconnected (114). From other experiments made with the 5-inch quantity- and intensity-armatures, it appeared that just as the amount of electrodynamic effect at N was augmented by increasing the intensity of the current when the ends C, D were disconnected, so also was this augmentation of the electrodynamic effect at N attended by a reciprocal diminution of the amount of current arriving at C, D. On the other hand, just as the electrodynamic effects at N were diminished by lowering the tension of the current when the ends C, D were disconnected, so also was this diminution of the electrodynamic effect at N attended by a reciprocal increase of the amount of current arriving at C, D.

186. I have already said that when the two copper conductors were extended in the sea with the ends C, D disconnected, the current from the $2\frac{1}{2}$ -inch machine was sufficient to melt 4 inches of thin wire at N; whereas the current from a single Grove's cell, connected with the same arrangement of conduc-

tors, would not make the shortest length of the same wire red-hot (176, 177).

187. That the increase of discharge from the $2\frac{1}{2}$ -inch machine into the sea above that which was transmitted into the canal from the same electromotor, under like conditions, should be attended by a corresponding diminution of electrodynamic effect at the distant point C, D, was only what might have been expected from the results already obtained in the canal: thus, when the current from this machine was transmitted through the conductors extended in the sea, the amount of current arriving at C, D would only make $\frac{3}{4}$ of an inch of the thin wire red-hot; whereas when the conductors were extended in the canal, the current from the same machine would melt, at C, D, 3 inches of this same wire. When, however, a single Grove's cell was connected with the conductors extended in the sea, the current arriving at the distant point was, owing to its comparatively low intensity, so little diminished that it would make $\frac{1}{2}$ an inch of the thin wire red-hot.

188. The similarity of the results exhibited at the distant ends of the conductors when extended in the sea and in the canal with those obtained with the double coil immersed in the tub of salt water, affords another illustration of the dependence of the phenomenon of the earth's conductivity upon the electrochemical properties of the liquids in contact with the conductors, and leads me to think that currents of electricity of great quantity, but of an intensity below that which is required to effect the electrolyzation of the liquid in which the conductors are submerged, might be transmitted to considerable distances without the necessity of surrounding the conductors with an insulating envelope. How far it may be practicable, under the most favourable circumstances, to transmit to distant places electric currents sufficiently powerful to be made available for any useful purpose under conditions so anomalous as those above mentioned, it is impossible to say without further experiments with conductors of much greater length than those which I have employed. I will, however, just mention that in a series of experiments made with two lengths of the same copper rope as that used in the preceding experiments, each 750 feet long, extended in the canal, while a marked diminution of electrodynamic effects was observed at C, D when currents of high tension were transmitted through these conductors, yet currents of low tension arrived at C, D with but little diminution of their primitive intensity. Even with a current of so great an intensity as that from the 10-inch intensity-armature it was very interesting to witness the powerful electrodynamic effects produced at the ends C, D of the naked conductors; for after the current had passed

through a body of water 750 feet in length, a sufficient quantity still remained to produce a brilliant electric light, and to melt 22 inches of iron wire .050 of an inch in diameter. With the current of lower intensity from the 5-inch quantity-armature, which melted at C, D 12 inches of iron wire .035 of an inch in diameter when the two 750-foot lengths of conductor were extended in air, a sufficient quantity of the current still remained to melt 7 inches of the same wire (.035 of an inch in diameter) at C, D after it had traversed a body of water 750 feet in length.

§ 4. *On the Transmutable Nature of Water.*

189. In everything but the exhibition of the electrolytic products themselves, we have seen that all the phenomena attending the discharge of the electromotors through liquids contained in insulated vessels were identical in kind with those exhibited in similar liquids in contact with the earth, but with the remarkable difference before treated upon, viz. that while in the former case the electrolyzation of the liquids is dependent upon the mutual action of the electrodes on each other through the intervening electrolyte, in the latter case no mutual action of the electrodes is necessary for the electrolyzation of these same liquids when in contact with the terrestrial bed. Now the establishment of this distinction between the two actions completely overturns those electrochemical theories which make the transmission of an electric current through an electrolyte dependent upon a series of decompositions and recompositions of the liquid extending all the way between the two electrodes.

190. Nor is the phenomenon of the independent action of the electrodes, when in contact with the terrestrial bed, less fatal to the generally received doctrine of the atomic composition of water than it is to the electrochemical theories above referred to. For if the oxygen evolved at one electrode be independent of the hydrogen evolved at the other, then would it appear that water is the ponderable base of both oxygen and hydrogen, and might therefore be transformed either into oxygen or into hydrogen alone.

191. Consequences of such vital importance to chemical science as those which I have just indicated rendered it imperatively necessary that the fact of the evolution of the electrolytic products at the electrodes in contact with the earth should not rest on an inference only, so long as the proof was within the reach of experiment. I therefore made an arrangement by which the electricity in its passage into the earth should electrolyze the water in contact with it in a visible manner, and at the same time enable me to collect the electrolytic products for the purpose of examination.

192. The principal electromotor used in these experiments was the 10-inch intensity-armature, the currents from which were turned all in the same direction by means of a commutator. The current from this armature, as I have already said, would melt in short circuit 7 feet of iron wire .065 of an inch in diameter.

193. About 300 feet of naked copper rope, T, fig. 16, such as was used in the previous experiments (103), were extended in the canal in a single length, and connected with the aërial conductor I, for the purpose of obtaining a good discharge from the pole F of the machine.

194. The other aërial conductor, H, connected with the pole G, instead of terminating in the canal in the same manner as the conductor I, terminated in a pool of fresh water, U, formed in the ground about 200 feet from the canal. This pool was about 30 inches in diameter and 10 inches deep.

195. A thin deal box, V, 1 foot square and 8 inches deep (into the construction of which no metal was allowed to enter), was fitted up as a pneumatic trough for the purpose of collecting the electrolytic products in tubes or jars. This trough was floated in the pool and then filled with fresh water.

196. The free extremity of the aërial conductor H was terminated with a piece of thick copper wire well insulated with gutta percha, and the end of this copper wire was again terminated with a strip of platinum $3\frac{1}{2}$ inches long and $\frac{1}{2}$ an inch wide. The strip of platinum was introduced into the open end of a eudiometer-tube 18 inches long and $\frac{1}{2}$ an inch internal diameter, graduated on its outer surface into hundredths of a cubic inch.

197. When contact was made between the poles of the electromotor and the arrangement of conductors above described, so long as the platinum termination of the aërial conductor H was not allowed to touch the earth or the water in the pool, no indication of the passage of a current appeared on the galvanometer at N, nor in any other manner. But directly the platinum electrode touched the water, either in the pool or in the pneumatic trough, the galvanometer indicated the passage of a current into the earth, while a stream of small bubbles of gas rose from the electrode to the upper end of the inverted eudiometer-tube, which had been previously filled with water. These bubbles of gas, when collected in sufficient quantity, were examined and found to be hydrogen.

198. On reversing the polar connexions at the machine, so that the platinum termination of the conductor H formed a positive instead of a negative electrode, the gas which collected at the top of the eudiometer-tube was now found to be oxygen.

199. A few measures of sulphuric acid were now mixed with

the water in the pool and in the trough for the purpose of increasing the conductivity of the water, and thereby facilitating the discharge of the electromotor into the earth.

200. When contact with the electromotor was made in such a manner that the platinum termination of the conductor H formed a positive electrode, 0.36 of a cubic inch of oxygen was generated in the eudiometer in one minute.

201. On reversing the polar connexions at the electromotor, so that the platinum should now form a negative electrode, 0.73 of a cubic inch of hydrogen was generated in the eudiometer in one minute.

202. A large platinum plate was now substituted for the small strip of platinum at the extremity of the conductor H, and the eudiometer-tube was replaced by a glass jar, into which the platinum plate was introduced.

203. On transmitting the current from the machine into the earth so that the platinum plate formed a positive electrode, the magnitude of the current was sufficient to melt 3 feet of iron wire .035 of an inch in diameter at N, and to generate, in six minutes, 20 cubic inches of oxygen, which represent the transmutation of 6.92 grains of water into the same number of grains of gaseous oxygen at the ordinary atmospheric pressure and temperature.

204. On reversing the polar connexions, so that the platinum now formed a negative electrode, 20 cubic inches of hydrogen were generated in three minutes, which volume of gas represents the transmutation of 0.43 of a grain of water into the same weight of gaseous hydrogen.

205. These experiments, besides exhibiting in the most striking manner the electrolyzation of water when in contact with the terrestrial bed, also show, by the volumes of oxygen and hydrogen generated in equal times, and by the times occupied in the evolution of constant volumes, that these gases were evolved at the electrodes in the proportions in which they enter into combination to form water.

206. In order that no reasonable doubt may be entertained that the oxygen or hydrogen evolved from the electrode in the pool had no relation whatever to the oxygen or hydrogen liberated on the surface of the conductor extended in the canal, I will just add another observation to those already made in proof of the independent action of the electrodes when plunged into masses of liquid of planetary dimensions. In the summer of 1861 I had the good fortune to witness, near Manchester, the effects of a discharge of lightning on a piece of copper wire which had been used by my direction for the purpose of establishing a connexion between a line of telegraph and the signal-

ling-instruments. One part of the copper wire happened to be placed in close proximity to the copper lightning-conductor of a factory chimney, while another part of the same wire passed within an inch or so of touching a thick iron gas-pipe connected, of course, with the underground mains. The lightning, it would seem, in its passage between the clouds and the earth, finding a much easier path by way of the copper wire and gas mains than by way of the lower extremity of the lightning-rod terminating in the moist earth, melted and volatilized a length of about 15 feet of that portion of the copper wire which extended between the lightning-conductor and the gas-pipe.

207. Now the power which atmospheric electricity, or electricity of tension, possesses of effecting the electrolyzation of water is so well established* that no reasonable doubt can be entertained that the fusing of the copper wire in the instance above mentioned was attended by the electrolyzation of the water in contact with the metal gas mains, in the same manner as the current from the 10-inch intensity-armature, which fused 3 feet of iron wire at N also, simultaneously electrolyzed the water in the pool. But in the case of the electrolyzation of water at the lower extremity of a lightning-rod, the very foundation of any mutual action which might be supposed to exist between the positive and negative electrodes of an electromotor discharging itself into the earth is entirely removed.

208. Moreover, as the electrolyzation of the water in contact with the inferior extremity of a lightning-conductor is independent of the length of conductor extending upwards above the surface of the earth, it would follow that if it were possible to prolong this conductor through space to a planetary body (Mars, for example) containing the same electrolyte as that forming part of the terraqueous globe, or if such a line of metal were extended between two aqueous spheres of planetary dimensions (fig. 17), then would a current of electricity traversing this line of metal (according to the polarity of the electrodes) transmute the water of each of these spheres either into oxygen or into hydrogen alone.

209. Though I have been at considerable pains to prove absolutely that the passage of an electric current from a conductor

* M. Van Troostwyk, "Sur la résolution de l'eau en gaz oxygène et hydrogène par l'étincelle électrique," *Annales de Chimie*, tome v. p. 276, 1790. Barry, "On the Chemical Action of Atmospheric Electricity," *Phil. Trans.* 1831, p. 165. Faraday, "Electrochemical Decompositions by Common Electricity," *Phil. Trans.* 1833, pp. 34-676. Andrews, "On the Polar Decomposition of Water by Common and Atmospheric Electricity," *British Association Report*, 1855.

into water is in every case accompanied by electrolyzation, yet I have nowhere ventured to affirm that it is impossible for water to conduct an electric current without electrolyzation when no electrode is present; since it is only at the surfaces where the electrode and the electrolyte touch each other that the electrolytic phenomena are really manifested, and, so far, the presence of an electrode composed of a substance heterogeneous to that which is in course of electrolyzation is essential to the absoluteness of the law of definite electrolysis. Now there is no evidence whatever to show that water, whether in insulated vessels or in contact with the earth, conducts an electric current after it leaves the electrode in any way different from that in which the current is propagated in solid bodies such as metals; and consequently no hypothesis founded upon a contrary assumption has any claim to be admitted. When, therefore, the positive or negative electrode of an electromotor is plunged into a body of water in contact with the earth, it seems to me that the corpuscular motion of the electrode, by the very act of communicating itself to the molecules of water in immediate contact with it, transforms them into molecules of oxygen or hydrogen, the electric impulse (after leaving the electrode) being then received into the entire mass of the terraqueous globe by a physical conductivity similar to that by which the current is propagated in metallic bodies.

210. It is evident that the conclusions which I have arrived at respecting the transmutation of water, either into oxygen or into hydrogen, when this liquid is in contact with the earth, and also with regard to the mode in which the current is propagated after leaving the electrode, must extend to the mode of propagation of the current and to the electrolyzation of water contained in insulated vessels.

211. The possibility of generating and conducting an electric current in a closed ring or circuit of water without the intervention of electrodes of any kind by a physical conductivity similar to that by which a current is generated and propagated in a circuit entirely metallic has already been proved by Faraday, who, by coiling an india-rubber tube filled with acidulated water round the armature of an electromagnet, and then magnetizing and demagnetizing the armature, succeeded in generating alternate induction-currents in the aqueous circuit, just as if the armature and helix of water formed part of a magneto-electric machine*.

212. MM. Van Breda and Logeman have pointed out that, even in this case, when the generating aqueous circuit was completed by the wire of a galvanometer as described by Faraday, electrolyzation occurs at the extremities of the wire in contact

* Philosophical Magazine, S. 4. vol. vii. p. 265.

with the liquid coil*; and since the arrival at the galvanometer of the induction-current must necessarily have been preceded by its generation in the convolutions of the aqueous conductor surrounding the armature, this experiment affords another proof that the presence of an electrode, whether employed in transmitting or in receiving an electric current, is essential to the production of electrolytic phenomena. But in a modification of this experiment which I have made with a water helix formed in an india-rubber tube 37 feet in length, 20 feet of which were coiled round the electromagnet, while the other 17 feet were used simply as a conductor, after removing the galvanometer-wires which had first indicated the generation of the induction-current, and plunging the ends of the india-rubber tube into a glass vessel containing the same acidulated water as that which filled the tube, in order that the induced current should now be generated and propagated in a closed homogeneous circuit of water returning into itself, it is manifest that though the induced current would be generated and conducted as certainly as when the galvanometer formed part of the circuit, yet, owing to the absence of electrodes, there could be no electrolyzation of the water in any part of the circuit. Hence, if Faraday failed to prove that a feeble electric current could be transmitted from an electrode into an electrolyte without electrolyzation, this great philosopher has at any rate the rare merit of having indicated the only method by which a current can be generated and propagated in an electrolyte without the intervention of electrodes, and consequently without electrolyzation.

213. From a further comparison of the phenomena attending the propagation of an electric current in an aqueous circuit with those observed in a metallic one (both closed and interrupted), it would appear that the electrolytic products which make their appearance at the point of interruption of an aqueous circuit such as we have just considered (212), are the result of the disruptive discharge of the electrolyte against the electrode, just as the spark or the electric light is the result of the disruptive discharge from metallic or graphite conductors through air,—the principal difference between the two discharges (and one which exhibits in a remarkable manner the transmutations of matter and force) being this, that whereas the ponderable particles of matter which are projected from solid conductors (such as metals or graphite) by the disruptive discharge give out the whole of their specific energies in the form of heat and light at the moment of discharge, the specific energies of the ponderable particles of matter which are projected from the water by the electrolytic discharge, on the contrary, remain suspended until they are

* *Philosophical Magazine*, S. 4, vol. viii. p. 465.

made to develop themselves at the will of the experimenter, either suddenly, by explosion, or gradually, in the form of the oxyhydrogen-flame.

214. As the electrolytic products, as we have seen, are only manifested and obtained at the point where the generating or conducting aqueous circuit is interrupted by an electrode, just as the spark only occurs at the point of interruption of the metallic circuit in air, so a theory framed upon the supposition that the transmission of an electric current through a closed metallic circuit depends upon a succession of invisible sparks following each other through the solid portions of the conductor, might be supported with the same amount of plausibility as an hypothesis based upon the assumption that the propagation of an electric current through a closed aqueous circuit (212), or through the homogeneous portions of a liquid, after the current leaves the electrode, is dependent upon a series of imperceptible decompositions and recompositions of the whole of the electrolyte traversed by the current.

215. That the evolution of electrolytic oxygen and hydrogen from water is an operation of transmutation, and not one of decomposition, was further shown by the converse experiment of the reconversion of oxygen or hydrogen alone into water. The strip of platinum terminating the conductor H (197) was introduced into an inverted test-tube $4\frac{1}{2}$ inches long and $\frac{1}{2}$ an inch internal diameter, the upper half of which was filled with hydrogen and the lower half with acidulated water. The lower end of the tube was then immersed in the pool of acidulated water (199); and the platinum electrode, exposed to the hydrogen and water in the tube, was made positive, by placing the conductor H in permanent connexion with the platinum terminal of a single cell of the nitric-acid battery; while the zinc terminal of the same cell was connected with the conductor extended in the canal.

216. After connexion between the battery-cell and the electrode in contact with the hydrogen and water in the pool had been maintained for about five hours, the water had risen in the tube one inch and a half, thereby indicating the transmutation of a corresponding volume of hydrogen into water.

217. On substituting an equal volume of oxygen for the hydrogen in the tube, and making the platinum electrode negative by a much more feeble current than that used in the preceding experiment, the water completely filled the tube in about thirty hours, thereby indicating the transmutation of the whole of the oxygen into water.

218. From these experiments we may justly infer that if a

feeble electric current were generated in any manner in a metallic conductor extending between two aqueous spheres of planetary dimensions, and if one of these spheres were surrounded by an atmosphere of oxygen and the other sphere by an atmosphere of hydrogen, then would the negative and positive electrodes terminating the extended conductor transmute the oxygen envelope of one sphere and the hydrogen envelope of the other sphere into water.

219. That the views which I have enunciated respecting the transmutation of water into oxygen or into hydrogen, and the new facts which I have adduced in support of them, should not have been previously touched upon in some way, would indeed have been surprising, considering the number of powerful intellects which have been engaged in the field of electrochemical science during the present century. Thus we find Davy, in the course of some experiments on the electrolyzation of water in contact with globules of mercury on which oxide was formed without the apparent liberation of free hydrogen, suggesting the same idea which I have been engaged in demonstrating, viz. that water might be the ponderable basis of both oxygen and hydrogen*; but he hesitates to adopt so formidable a conclusion, as in no other case could he obtain one gas without the corresponding quantity of the other, or without some product into which the other gas might be supposed to enter. Subsequent researches on the power which hydrogen in the nascent state has to combine with the oxides, have fully justified this suspense of judgment. But in following up this speculation in the case of the electrolyzation of water when the positive and negative electrodes of the powerful battery of 2000 double plates of the Royal Institution were plunged into separate vessels of water, connexion between them being established through the human body by inserting a finger in each vessel, this philosopher would hardly seem to have been just to his original conception; for had he not attempted to account for the phenomena by the theory of transfer, or been hampered with the hypothesis that the liberation of the oxygen and hydrogen in the separate vessels depended upon a series of decompositions and recompositions of the liquids extending all the way between the two electrodes, no other conclusion could have been arrived at but that which he hesitated to adopt.

220. Another natural philosopher, whose researches would seem to have led to results intimately connected with some of those which I have obtained, is Mr. Grove, who has pointed out in the most forcible manner an insuperable objection which the

* *Elements of Chemical Philosophy*, by Sir Humphry Davy, LL.D., 1812, vol. i. pp. 172, 485-488.

Phil. Mag. S. 4. Vol. 36. No. 241. Aug. 1868.

action of his gas battery presents to the electrochemical theory of Grothuss, which makes the propagation of the current between two electrodes dependent upon a series of decompositions and recompositions of the water through which the current is transmitted*. Grove's objection to the theory is this:—In a single pair of the gas battery we must, according to the theory of Grothuss, suppose that oxygen and hydrogen in their separate states tear asunder oxygen and hydrogen already united—the force which produces the composition of water being in fact regarded by the theory as sufficiently strong to overcome the force by which its constituents are already held together. Grove also points out that the gas battery presents cases in which, according to this theory, a more feeble affinity overcomes a more powerful one, as when water is the electrolyte, and binoxide of nitrogen and oxygen are the gases.

221. Now the action of the gas battery, while altogether inexplicable by the theory of Grothuss, is very simple when viewed in connexion with the new results which I have obtained, and with the dynamical theory of gases which has recently made such advances through the labours of Joule, Clausius, and others. For if free oxygen and hydrogen may now be looked upon merely as the same ponderable matter, separated into molecules of greater and less weight in a state of rapid motion, the lighter molecules (hydrogen) moving at a much greater velocity than the heavier molecules (oxygen), in order that equal volumes of these gases may produce equal pressures, it is no longer surprising, now that the influence which platinum exercises in inducing gaseous combination is known, that the molecules of gas should be able to communicate their motion to the comparatively inert molecules of water which are in contact with the surface of the platinum, in a similar manner to that in which the translatory motion of inelastic bodies is by collision transformed into the motion of heat, that portion of the water extending between the electrodes but not in immediate contact with them being not electrolyzed, but simply completing the circuit in the same manner as an ordinary metallic conductor.

222. That the conclusions which I have arrived at respecting the nature of water will escape the penalties usually visited upon opinions which run counter to established ideas, whether true or false, is hardly to be expected on a subject on which such strong views are held as on the composition of water. But for the benefit of those who, like myself, refuse to accept the explanation of any phenomenon as being absolutely true, without first considering it in relation to the mind of the individual who essays to interpret it, I will just observe that, in a critical examination of

* Philosophical Magazine, S. 3. vol. xxvii. p. 348.

the various notions which have up to the present time been held on the nature of water, it is highly important that a proper estimate should be taken of the powerful influence which the chronological order in which some discoveries in physical science are made has in establishing in the minds of philosophers of one generation ideas of causation and composition which are looked upon as absurd by philosophers of another. Thus we find that water, up to the time of Cavendish and Watt*, was considered to be an element, simply on account of the inability of chemists to convert it into anything else; while the present notion of the compound nature of water rests entirely on the inability of modern chemists to obtain water from oxygen or hydrogen alone, or to obtain either of these gases alone from water.

223. Now, had the chronological order in which the experiments described in this paper and those of Cavendish and Watt were made been reversed, the doctrine of the transmutable nature of water would at once have been accepted, as agreeing strictly with observation and experiment. But the theory of the atomic composition of water, if broached for the first time at the present day, would be summarily rejected as not accounting either for the evolution of oxygen or hydrogen from water forming part of the terraqueous globe (200, 201), or for the reconversion of either of these gases alone into water (216, 217).

224. Nor would the theory of Grothuss under these altered circumstances meet with any better reception at the present day than would be accorded to the theory of the composition of water, because the former theory, as we have seen, breaks down completely when called upon to account for the electrolyzation of planetary masses of water by means of a single electrode, or when the distance between the two electrodes is considerable; and yet, although the theory of Grothuss was invented solely for the purpose of reconciling the phenomenon of the electrolyzation of water with the theory of its composition †, and therefore ought not to be used in any manner to support the latter theory, there will not be found wanting some minds in which these two theories are so confusedly mixed together that Grothuss's theory will be brought forward to explain and support the theory of the composition of water, while the latter theory will, in its turn, be adduced as proof sufficient of the truth of the theory of Grothuss.

225. It is to philosophers of a breadth and liberality of mind sufficient to realize the full force of the accidental influences which I have just indicated that I look for an impartial consideration of this great question—to philosophers who are so far

* Philosophical Transactions, 1784, pp. 130, 329.

† "Théorie de la décomposition des liquides par l'électricité galvanique," par C. J. D. de Grothuss, *Annales de Chimie*, vol. lviii. p. 64 (1806).

faithful to the evidence of their own senses and to their own understandings as not to be unduly influenced by the truth as it appears to this man or to that, but who, with a singleness of purpose which is regardless of all consequences, seek to behold the truth only as it is in nature.

[Our correspondent's concluding paragraphs (222, 225) have induced us to publish his statements regarding the transmutable nature of water, although we do not agree with him, and believe that the results of his experiments will bear a very different interpretation. We are of opinion that the supposed conversion of water into oxygen or hydrogen (197 *et seq.*) is simply a decomposition of water, the oxygen being evolved in the one pool, the hydrogen in the other, *the earth acting as the conductor between the two pools*. The large surface of the conducting wire in the one pool would naturally make the amount of gas evolved at that pole appear very small. We also think that the conversion of hydrogen or oxygen into water admits of a different explanation; it seems to us that the author has simply formed a gas battery.—W. F.]

XV. *On the Theory of Pressure in Fluids.* By R. MOON, M.A.,
Honorary Fellow of Queen's College, Cambridge*.

IN a former paper† I treated of four cases of pressure, in the first of which the received theory as to the law of pressure in elastic fluids was shown to contradict a universally received principle, viz. the principle that action and reaction are equal and opposite. In the second case considered, the assumption of the truth of the same law was shown to lead to an absurd result; while in the two remaining cases the same assumption was shown to lead to results which are palpably erroneous.

If we inquire into the evidence upon which the received law of pressure in elastic fluids rests, it will, I think, be found that the law having been proved experimentally to hold in the case of equilibrium, it was assumed, in the absence of any obvious reason to the contrary, to hold generally; and certain illustrious analysts having shown that its adoption led to some striking and beautiful results, suspicion was lulled, and acquiescence in the correctness of the theory became general.

The cases considered in my former paper establish conclusively that the law which has thus been assumed is not universally true. A further inquiry into the subject therefore becomes necessary; and such an inquiry, so far as the case of motion in one direction is concerned, I propose to institute in the present paper. It will be found, if I mistake not, to lead to conclusions of the greatest interest, simplicity, and value.

* Communicated by the Author.

† See *Philosophical Magazine*, vol. xxxvi. p. 27.

Suppose that we have a cylindrical tube of indefinite length filled with air; and let x be the ordinate of the position of rest of a lamina made by planes perpendicular to the axis of the tube; y the same ordinate at the time t ,—the whole motion being supposed parallel to the axis of the tube, to which the ordinates are measured parallel.

Let ρ , v , p denote respectively the density, velocity, and pressure at the time t at the point whose ordinate at that time is y .

If D be the density of equilibrium, we shall have the following equation of motion, viz.

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \frac{dp}{dx}, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

an equation which holds without reference to any hypothesis as to the nature of the law of pressure, resting on the simple assumption that the fluid is a continuous substance.

It is obvious that in any particular case of motion each of the quantities p , ρ , v must be capable of being expressed in terms of x and t , *i. e.* that we may assume

$$p = f_1(xt),$$

$$\rho = f_2(xt),$$

$$v = f_3(xt).$$

From the last two, eliminating x , we may obtain

$$t = f_a(\rho, v);$$

and eliminating t , we may obtain

$$x = f_b(\rho, v).$$

Substituting these values in the equivalent of p , we shall obtain

$$p = \text{funct.}(\rho, v).$$

We may hence perceive the gigantic character of the assumption made in the received theory by taking, under all circumstances, $p = a^2\rho$.

All that we know of the pressure *à priori* is that p is a function of ρ and v , or, as we may put it, that

$$p = F_a(\rho) + F_b(\rho, v);$$

where F_b is some function of ρ and v which vanishes when $v=0$. Now, even if Mariotte's law established (which it does not*)

* Mariotte's law has only been proved to hold when the air is in equilibrium. In the last three cases of my former paper it is clear that the air is not in equilibrium at the time when we have $v=0$ throughout the entire mass, at which time it is shown in each case that Mariotte's law does not hold.

that under all circumstances when $v=0$ we have $p=a^2\rho$, that would only entitle us to assume that

$$p=a^2\rho+F_b(\rho, v),$$

where F_b is some unknown function of ρ and v which vanishes when $v=0$, the making $F_b=0$ when v does not $=0$ being to be regarded as a pure unqualified assumption.

Returning from this digression—since p may be regarded as a function of ρ and v , we shall have

$$\frac{dp}{dx} = \frac{dp}{d\rho} \frac{d\rho}{dx} + \frac{dp}{dv} \frac{dv}{dx} = R \frac{d\rho}{dx} + V \frac{dv}{dx} \quad \text{suppose,}$$

where R and V are functions of ρ and v only; and substituting this value in (1), observing that

$$v = \frac{dy}{dt}, \quad \rho = D \cdot \left| \frac{dy}{dx} \right|^{-1},$$

we get

$$0 = \frac{d^2y}{dt^2} + \frac{V}{D} \frac{d^2y}{dx dt} - R \left| \frac{dy}{dx} \right|^{-2} \frac{d^2y}{dx^2}, \quad \dots \quad (2)$$

in which, by virtue of the above equivalents for ρ and v , V and R may be considered as functions of $\frac{dy}{dt}$ and $\frac{dy}{dx}$ only.

In treating of the integration of this equation, I shall assume that V and R are *definite* functions, *i. e.* that they contain neither arbitrary constant nor arbitrary function. If, by reason of the expression for p in terms of ρ and v (and therefore in terms of $\frac{dy}{dt}$, $\frac{dy}{dx}$) involving arbitrary constants or arbitrary functions, such or the like constants or functions enter into the composition of (2) when exhibited in its most general form, I shall assume that definite values have been assigned to such constants and functions; so that it is only to the equation (2) when modified in the manner just indicated that the remarks immediately following must be conceived to apply.

Thus much being premised, I shall simply assume that (2) is integrable by Monge's method, and shall follow out some of the consequences of that assumption.

One consequence of that assumption will be that (2) is capable of being derived from a single partial differential equation of the first order, of the form

$$F\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right) = \phi \left\{ F_1\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right) \right\}, \quad \dots \quad (2a)$$

where ϕ is arbitrary; whence it follows, putting $\phi = \alpha$, where α is any constant, that (2) is capable of being derived from an equation of the form

$$F\left(xyt \frac{dy}{dx} \frac{dy}{dt}\right) = \alpha, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where α is an arbitrary constant independent of the value of any constant appearing in F .

Differentiating (3), we get

$$\left. \begin{aligned} 0 &= F'(x) + F'(y) \frac{dy}{dx} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx^2} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dx dt}, \\ 0 &= F'(t) + F'(y) \frac{dy}{dt} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx dt} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dt^2}, \end{aligned} \right\} \quad . \quad (4)$$

By hypothesis, (2) is derivable from these last two equations and from (3); and since α appears in (3) and does not appear in (2) or in either of the equations (4), it is clear that (2) must be derivable from equations (4) alone, without taking account of (3).

Hence the right side of (2) must be identical with the right side of the following equation, viz.

$$\begin{aligned} 0 &= A \left\{ F'(x) + F'(y) \frac{dy}{dx} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx^2} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dx dt} \right\} \\ &\quad + B \left\{ F'(t) + F'(y) \frac{dy}{dt} + F'\left(\frac{dy}{dx}\right) \frac{d^2y}{dx dt} + F'\left(\frac{dy}{dt}\right) \frac{d^2y}{dt^2} \right\}, \end{aligned}$$

where A, B are functions of $txy \frac{dy}{dx} \frac{dy}{dt}$. Therefore, comparing the coefficients of corresponding terms, we must have

$$\begin{aligned} 1 &= BF'\left(\frac{dy}{dt}\right), \\ \frac{V}{D} &= BF'\left(\frac{dy}{dx}\right) + AF'\left(\frac{dy}{dt}\right), \\ -R \left(\frac{dy}{dx}\right)^{-2} &= AF'\left(\frac{dy}{dx}\right), \\ 0 &= B \left\{ F'(t) + F'(y) \frac{dy}{dt} \right\} + A \left\{ F'(x) + F'(y) \frac{dy}{dx} \right\}; \end{aligned}$$

whence we have, eliminating A and B ,

$$\left. \begin{aligned} 0 &= \frac{F' \left(\frac{dy}{dx} \right)^{-2}}{F' \left(\frac{dy}{dt} \right)} - \frac{V}{D} \cdot \frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} - R \left[\frac{dy}{dx} \right]^{-2}, \\ 0 &= \frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} - R \left[\frac{dy}{dx} \right]^{-2} \cdot \frac{F'(x) + F'(y) \frac{dy}{dx}}{F'(t) + F'(y) \frac{dy}{dt}}, \end{aligned} \right\} \dots \dots (5)$$

the first of which gives us

$$\frac{F' \left(\frac{dy}{dx} \right)}{F' \left(\frac{dy}{dt} \right)} = \frac{V \pm \sqrt{V^2 + 4RD^2} \left[\frac{dy}{dx} \right]^{-2}}{2D} \dots \dots \dots (6)$$

Let

$$F \left(xyt \frac{dy}{dx} \frac{dy}{dt} \right) = F \left(xyt \frac{D}{\rho} r \right) = f(xyt\rho r),$$

then

$$F' \left(\frac{dy}{dx} \right) = F' \left(\frac{D}{\rho} \right) = -\frac{\rho^2}{D} f'(\rho),$$

$$F' \left(\frac{dy}{dt} \right) = f'(v), \quad F'(x) = f'(x), \quad F'(y) = f'(y), \quad F'(t) = f'(t);$$

therefore (6) gives us

$$0 = f'(\rho) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot f'(v); \quad \dots \dots \dots (7)$$

while from (5) we get, reducing,

$$\begin{aligned} \frac{f'(x) + f'(y) \frac{D}{\rho}}{f'(t) + f'(y)v} &= \frac{D}{2R\rho^2} (V \pm \sqrt{V^2 + 4R\rho^2}) \\ &= -\frac{2D}{V \mp \sqrt{V^2 + 4R\rho^2}}, \end{aligned}$$

or

$$\left. \begin{aligned} 0 &= \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} f'(x) + f'(t) \right) \\ &+ \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) f'(y); \end{aligned} \right\} \dots \dots \dots (8)$$

f must satisfy (7) and (8) simultaneously.

If $\omega_1 = C_1$ and $\omega_2 = C_2$ be the respective integrals of the equations

$$\left. \begin{aligned} 0 &= dv - \frac{V + \sqrt{V^2 + 4R\rho^2}}{2\rho^2} d\rho, \\ 0 &= dv - \frac{V - \sqrt{V^2 + 4R\rho^2}}{2\rho^2} d\rho, \end{aligned} \right\} \dots \dots (8a)$$

we shall have in ω_1, ω_2 two values for f , since they obviously satisfy (7) and also, being free from x, y , and t , satisfy (8).

Again, we have for the integration of (8) the following auxiliary equations, viz.

$$\begin{aligned} 0 &= dx - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} dt, \\ 0 &= dy - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) dt, \\ 0 &= d\rho, \\ 0 &= dv. \end{aligned}$$

Hence we shall have the two following additional values for f , viz.

$$f = \phi \left\{ \begin{aligned} &\rho, v, \left[x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right], \\ &\left[y - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) \cdot t \right], \end{aligned} \right\} \dots \dots (9)$$

where ϕ is arbitrary, provided these values satisfy also equation (7).

But if (9) satisfies (7), the latter will be satisfied by

$$f = \phi \left\{ x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\},$$

in order to which we must have

$$\left. \begin{aligned} 0 &= \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) \\ &+ \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}); \end{aligned} \right\} (10)$$

or, which is the same thing,

$$\left. \begin{aligned} 0 &= (V \mp \sqrt{V^2 + 4R\rho^2}) \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) \\ &- 2R \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}), \end{aligned} \right\} \dots (10a)$$

which, taking alternately the upper and lower signs, constitutes a pair of equations of condition which must be satisfied when f is susceptible of both the values (9).

Moreover, since if (9) satisfies (7) the latter will be satisfied by

$$f = \phi \left\{ y - \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) t \right\},$$

we have the further equations of condition,

$$0 = \frac{d}{d\rho} \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} \left(\frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2\rho} + v \right) \Big\},$$

which reduce to the following, viz.

$$0 = \pm \frac{2\sqrt{V^2 + 4R\rho^2}}{\rho} + \frac{d}{d\rho} (V \mp \sqrt{V^2 + 4R\rho^2}) + \frac{V \pm \sqrt{V^2 + 4R\rho^2}}{2\rho^2} \cdot \frac{d}{dv} (V \mp \sqrt{V^2 + 4R\rho^2}).$$

Hence if (10) hold, we must have

$$V^2 + 4R\rho^2 = 0,$$

a conclusion which may be rejected on account of defect in generality*. Therefore when equations (10) hold, (9) must reduce itself to

$$f = \phi \left\{ \left(x - \frac{V \mp \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right), \rho, v \right\}, \quad . \quad . \quad (11)$$

and we shall thus have for f four values in all, viz. those given by the last equation together with those previously found, viz. ω_1, ω_2 .

Now it will be remembered that the original integral which we assumed for (2) was (2 a), or, which is the same thing,

$$f(xyt\rho v) = \chi \{ f_a(xyt\rho v) \}. \quad . \quad . \quad . \quad (12)$$

It is clear, however, that this last equation may be put under the form

$$f_a(xyt\rho v) = \chi_{-1} \{ f(xyt\rho v) \},$$

where χ_{-1} denotes an arbitrary function; and if we treat this equation in the same way in which we have treated (12), we shall arrive at precisely the same formulæ for the determination

* The grounds upon which I rest this conclusion will hereafter be more distinctly pointed out.

of f_a as those at which we have already arrived for the determination of f .

Thus the occurrence of two of the four values of which f has been shown to be susceptible is at once accounted for; and if we reflect that almost universally when an equation of the second order is integrable by Monge's method, there are two equations of the first order of the form (2 a) from each of which it is separately derivable, there cannot, I think, be a doubt that the four values above derived for f must be attributable to the values of f and f_a in equation (12), and to the corresponding functions in the other first integral of (2).

Acting from this clue, we shall be justified in assuming, and we shall find it to be the fact, that when the equations of condition (10) are satisfied, (2) will be susceptible of the two following integrals, from each of which separately it is capable of being derived, viz.

$$\left. \begin{aligned} \omega_1 &= \phi \left\{ x - \frac{V - \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\}, \\ \omega_2 &= \psi \left\{ x - \frac{V + \sqrt{V^2 + 4R\rho^2}}{2D} \cdot t \right\}. \end{aligned} \right\} \quad . \quad . \quad (13)$$

If we now recur to the equations of condition (10) or (10 a), or, as they may be written,

$$\left. \begin{aligned} 0 &= P_2 \frac{dP}{d\rho} - 2R \frac{dP_2}{dv}, \\ 0 &= P_1 \frac{dP_1}{d\rho} - 2R \frac{dP_1}{dv}, \end{aligned} \right\} \quad . \quad . \quad . \quad (14)$$

where

$$P_1 = V + \sqrt{V^2 + 4R\rho^2}, \quad P_2 = V - \sqrt{V^2 + 4R\rho^2},$$

the form of the equations leads us to conclude that, if neither of the quantities $P_1 P_2$ vanishes (as for instance P_1) so as to render $\frac{dP_1}{d\rho} = \frac{dP_1}{dv} = 0$, we shall have

$$P_1 = P_2,$$

which implies that we have $\sqrt{V^2 + 4R\rho^2} = 0$,—a conclusion which I have already pointed out as one to be rejected as deficient in point of generality*.

* This may be seen more distinctly as follows. In the case supposed, (13) become

$$\omega_1 = \phi \left\{ x - \frac{V}{2D} t \right\}, \quad \omega_2 = \psi \left\{ x - \frac{V}{2D} t \right\},$$

whence we get $\omega_1 = \text{funct. } \omega_2$; and therefore we must have $\rho = \text{funct. of } v$, since $\omega_1 \omega_2$ involve ρ and v only. Now not only is this conclusion, viz. that $\rho = \text{funct. } v$, clearly defective in point of generality, but, if it were true, we

But if one of the quantities P_1, P_2 is constant—for instance, if we have

$$V + \sqrt{V^2 + 4R\rho^2} = 2\alpha,$$

where α is constant, we shall have

$$R\rho^2 = \alpha^2 - \alpha V,$$

or

$$0 = \rho^2 \frac{dp}{d\rho} + \alpha \frac{dp}{dv} - \alpha^2,$$

which gives us

$$p = -\frac{\alpha^2}{\rho} + \chi\left(v + \frac{\alpha}{\rho}\right),$$

a value which, it will be found, satisfies *both* the equations of condition (14).

In this case equations (8 a), from which ω_1, ω_2 are to be derived, become

$$0 = dv - \frac{\alpha}{\rho^2},$$

$$0 = dv - \left\{ \chi' \left(v + \frac{\alpha}{\rho} \right) - \alpha \right\} \frac{d\rho}{\rho^2}; \quad . \quad . \quad . \quad (15)$$

$\therefore \omega_1 = v + \frac{\alpha}{\rho}$, and equations (13) become

$$v + \frac{\alpha}{\rho} = \phi \left\{ x - \frac{\chi' \left(v + \frac{\alpha}{\rho} \right) - \alpha}{D} t \right\},$$

$$\omega = \psi \left\{ x - \frac{\alpha}{D} t \right\},$$

where $\omega = \text{const.}$ is the integral of (15).

If we put $u = v + \frac{\alpha}{\rho}$, the three equations which determine the circumstances of the motion, *i. e.* the pressure, density, and velocity, become

$$\left. \begin{aligned} p &= -\frac{\alpha^2}{\rho} + \chi(u), \\ u &= \phi \left\{ x - \frac{\chi'(u) - \alpha}{D} t \right\}, \\ \frac{1}{\rho} + \int \frac{du}{\chi'(u) - 2\alpha} &= \psi \left\{ x - \frac{\alpha}{D} t \right\}, \end{aligned} \right\} . \quad . \quad (16)$$

might assume $p =$ a function of ρ only—an assumption which is open to all the objections which in my former paper have been shown to apply to the law of the received theory, *viz.* $p = a^2\rho$.

results which have been evolved from the general equation (2) without the aid of any subsidiary hypothesis with regard to the nature of the law of pressure, by the simple application to (2) of the ordinary process for the integration of partial differential equations of the second order involving two independent variables*.

If the fact of the expression for the pressure containing an arbitrary function, thus causing *three* arbitrary functions to enter into the complete solution of the problem, occasions surprise, I may observe that it may be shown *à priori* that such must be the case.

6 New Square, Lincoln's Inn,
July 7, 1868.

XVI. *On the Diammonic Carbonate, or Normal Carbonate of Ammonium.* By EDWARD DIVERS, M.D., F.C.S., Lecturer on Natural Philosophy, Charing Cross Hospital†.

IT is now nearly thirty years ago since H. Rose wrote, in his classical memoir on the compounds of ammonia with carbonic acid‡, “the neutral anhydrous carbonate of ammonia cannot be combined with the quantity of water sufficient to convert the ammonia into oxide of ammonium.” The carbonates of ammonia have since been reinvestigated by Deville§; and he admits the existence of only two crystalline combinations—the tetrammonic dihydric tricarbonat (or true sesquicarbonat), and the ammonic hydric carbonat (or bicarbonat).

Resting on the authority of these distinguished chemists, the text-books tell us that the carbonat of ammonium *cannot be isolated*. I have now to announce that the normal or diammonic carbonat can be prepared in the simplest manner imaginable. I must say that, with the above assertions before me, I was much pleased to succeed very easily in my efforts to form this body; but I certainly was not astonished, because comparatively recently it has been shown by Dr. Hofmann that two bodies whose place is little beyond the threshold of chemistry (formic aldehyd and formamid), and which it was said could not be produced, are actually to be prepared in the most ordinary manner.

Diammonic carbonat is formed by treating the commercial

* The truth of equations (16) may be readily verified, observing that, in virtue of the value there given for p , (2) can be put under the form

$$0 = \frac{du}{dt} + \frac{\chi'(u)}{D} \frac{du}{dx} + \frac{\alpha}{\rho^2} \frac{dp}{dt} + \frac{\alpha^2}{D\rho^3} \frac{dp}{dx}.$$

† Communicated by the Author.

‡ Pogg. Ann. vol. xlv. p. 373.

§ Ann. de Chim. S. 3. vol. xl. p. 87.

carbonate with a solution of ammonia. Some of this substance dissolves in the ammonia-water; the rest remains, as a skeleton of the original solid, in the condition of a soft mealy semicrystalline mass. This is the normal carbonate. If it be digested for some days in a closed vessel with the ammoniacal liquor after this has had its free ammonia renewed in it by the passage of some ammonia gas through it while kept cool by external means, a remarkable phenomenon is observed. Even when the solid is at first in such quantity as to leave the mixture only semifluid, the whole will gradually, particularly when occasionally agitated, become a solution. Warming the vessel in the early period of the digestion seems to have little effect in hastening this peculiar solution; while cooling it in ice has little effect (if any) in increasing the solidification of the mixture. This interesting occurrence of the slow disappearance of the solid carbonate I shall not in the present paper attempt to explain (though I am quite satisfied that I am able to do so); for my experiments in this direction are, as yet, incomplete. If now to the clear liquid more of the commercial carbonate be added, further solution of it occurs; and now a gentle heat seems favourable to this process. Either on cooling after warming, or by the action of applied cold, fine spicula fill the liquid, almost solidifying it from the commencement of their formation, though, somewhat like the potassic silicofluoride, they do not much diminish the transparency of the whole. These minute crystals, which are identical in composition with the mass left when the commercial carbonate is treated with ammonia, rapidly increase in quantity, but not materially in individual size. They are not grouped in stars or bundles, but diffused uniformly through the liquid. By jolting the vessel, the crystals, if not too numerous, may be shaken together so as to form a shrunken mould, as it were, of the inside of the vessel. Removed from their mother-liquor, or, more correctly, drained from and squeezed free of their mother-liquor, they begin to decompose. But with proper precautions to prevent this decomposition, they can be exhibited in soft masses of minute crystals of brilliant silky lustre. They smell most intensely of ammonia. They dissolve very freely in water, but require about seventy measured parts of ordinary rectified spirit to dissolve them. In strong ammonia solution they dissolve at first very sparingly; but solution continues to go on slowly, as I have above described it doing in the case of the carbonate in the mealy condition. Exposed freely to the air, the salt entirely loses its lustre in a few moments, evolving torrents of ammonia, and becoming at the same time damp from the liberation of water. After a while (very rapidly if disturbed and repeatedly pressed between fresh bibu-

lous paper) it becomes a white powder of the acid carbonate, weighing little more than half the original substance—the matters volatilized, therefore, being not only ammonia and water, but also a good deal of the entire salt or its constituents. Not only do the crystals become damp in the open air, but also, and rapidly, when they are in closed vessels.

The diammonic carbonate in the same crystalline condition may also be obtained by dissolving the commercial salt to saturation in water and then passing ammonia gas through the solution kept cool. A moderately strong aqueous solution of the new salt, cooled in ice, does not crystallize; but when treated with ammonia gas, the salt crystallizes out from it. By digestion afterwards in the ammoniacal liquor the crystals gradually disappear, and do not return when the liquid is cooled again to 0° C. Gently heated in a closed tube, the crystals melt to a transparent liquid, which volatilizes and forms a semicrystalline moist sublimate. If the fused mass be cooled, it gradually solidifies to a mass of prisms; this mass, and most of the sublimate, spontaneously volatilize when the tube is kept open; at least they did so on the occasion in which they were thus tried.

To prepare the salt for analysis, it was rapidly deprived of the bulk of the mother-liquor by draining on bibulous paper, and then submitted to moderate pressure between fresh layers of paper. In the case of sample I., after draining the crystals on paper, they were enclosed in a small close glass chamber (extemporized for the purpose) and packed tightly between bibulous paper, and the whole was then left for some time surrounded with ice. The vessel had to be opened from time to time to renew the paper and break down the flakes of crystals; but these operations were performed with all possible expedition. When the drying appeared to be complete, the soft crystalline cake of still lustrous crystals was removed, pressed for a moment firmly between a fresh fold of paper (no distinct damping of which was observed), and then rapidly transferred to weighing-tubes and forthwith analyzed. By the time the substance was in the tubes it had lost much of its crystalline lustre, and began to be slightly adherent from acquired dampness, so rapidly does the salt change.

Sample II. was not enclosed in the glass chamber and kept cool by external cold; but the active volatilization of its highly ammoniacal mother-liquor maintained for a time a sensibly low temperature in the mass. Some difference in behaviour during the drying was observed, in consequence of this different arrangement. After a few changes of paper little progress seemed to be made in the drying; and besides, the paper began to adhere somewhat closely to the cake, which also lost most of its lustre. The cause of these phenomena was undoubtedly the decomposi-

tion of some of the salt, attended with the liberation of water, and a deposition of the little-soluble acid carbonate on the surface of the cake and in the pores of the paper. Not to lose too much ammonia, the salt was collected in a slightly damp condition. Sample III. was the same preparation as II., but was subjected to a much longer process of drying, with, however, little apparent advantage.

The substance IV. was the semicrystalline solid which is left when the commercial carbonate is first treated with ammonia solution. It was dried as was II., and behaved similarly, except that it had no lustre to lose. Lastly, V. was the crystalline form of the carbonate obtained by dissolving the commercial carbonate (freed from adhering acid carbonate) to saturation in water mixed with some strong solution of ammonia, and then keeping the solution cool for a couple of hours in ice. It was not prepared with much dispatch for analysis; and the numbers obtained show evidence of marked decomposition having occurred. In sample I., which appears to have contained over 2 per cent. moisture, the ammonia will be seen to have been almost exactly in quantity equivalent to that of the carbonic anhydride. The portion of II. destined for the determination of the ammonia was spoiled.

If either of the forms of the carbonate be leisurely pressed with free exposure, so long as it damps fresh paper it becomes the dry hydric ammonic (or acid) carbonate. One specimen thus obtained was analyzed.

The ammonia was determined by standard solutions of sulphuric acid and soda, the carbonate being dropped from the weighing-tube into a flask containing a small excess of the acid diluted, the solution boiled and shaken to get rid of all carbonic anhydride, and then the soda added to neutralization. The carbonic anhydride was estimated in the direct way, after its expulsion by hydrochloric acid and passage over fused calcic chloride and blue-vitriolized pumice, by combining it with soda-lime in a U-tube. The apparatus was arranged and the operation performed with the precautions prescribed in Fresenius's 'Quantitative Analysis.'

Sample I.—7877 grm. gave ·2948 grm. carbonic anhydride ;
·74565 grm. gave ·21318 grm. ammonia.

Sample II.—7063 grm. gave ·2715 grm. carbonic anhydride.

Sample III.—7172 grm. gave ·2857 grm. carbonic anhydride ;
·5483 grm. gave ·1547 grm. ammonia.

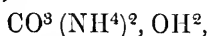
Sample IV.—1·05965 grm. gave ·4042 grm. carbonic anhydride ;
·4627 grm. gave ·12886 grm. ammonia.

Sample V.—85645 grm. gave 3353 grm. carbonic anhydride ;
 53245 grm. gave 1411 grm. ammonia.

These data give the following percentage composition for the five several preparations :—

	I.	II.	III.	IV.	V.
Carbonic anhydride.	37·43	38·44	39·84	38·15	39·15
Ammonia . . .	28·59	..	28·21	27·85	26·50

The formula of the diammonic carbonate with one atom of water of crystallization,



gives the following numbers :—

Carbonic anhydride . .	38·60
Ammonia	29·82
Water	31·58

The powder into which the diammonic carbonate passes on exposure gave 21·66 per cent. ammonia, determined in 671 grm. Theory requires for $\text{CO}^3 \text{H} (\text{NH}^4)$ the number 21·52.

XVII. *On a modified Spectroscope for use in the examination of Minerals.* By J. EMERSON REYNOLDS, Member of the Royal College of Physicians, Edinburgh, Keeper of the Minerals and Analyst to the Royal Dublin Society*.

THE modified spectroscope presently to be described was arranged some time ago by me, for the purpose of supplying a deficiency long felt in using the ordinary form of spectrum-apparatus.

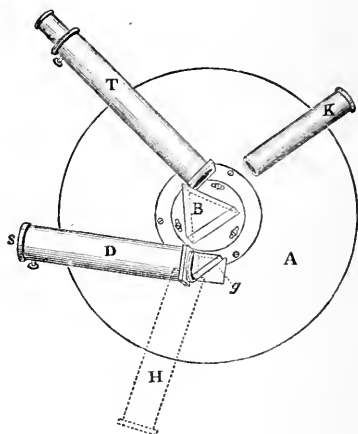
I believe all who have much employed the delicate and beautiful mode of investigation afforded by the method of spectrum-analysis, will agree with me that the ordinary single-prism arrangement is by no means well suited for use in the routine work of a chemical laboratory. When the eye of the operator is applied to the telescope of the instrument, the slit and the flame in which the test-specimen is being ignited are out of view and beyond easy control. Practice will, of course, overcome much of the difficulty attendant upon this circumstance; but for rapid and satisfactory work it is always necessary to obtain the aid of an assistant. The same objection applies with equal force to the direct-vision spectroscope of M. Hofmann, excellent though this instrument is in other respects. Every chemist knows how advantageous it is to have his apparatus entirely under his own

* Communicated by the Author, having been read before the Royal Dublin Society, December 16, 1867.

control; hence I set to work to remedy the defect above referred to, and have succeeded in arranging the spectroscope so as to admit of its use in ordinary analysis with great facility and comfort to the operator.

The accompanying woodcut represents the spectroscope as seen from above.

A is a circular table of brass surmounting a pillar and heavy foot, not shown in the drawing. In the centre of the table and on a raised metal disk the prism B is fixed securely, but the small carrier-plate admits of slight rotation round its centre in order that the adjustment of the prism may be easily effected; the latter is a glass cell filled with bisulphide of carbon, the refracting angle being 59° . The tube D is fitted to the table, and carries at one end the slit S and ad-



justing-screw, and at the other the collimating lens. On a little stage is fixed the reflecting prism *g*, the use of which will presently be shown. T is the observing-telescope, which has a magnifying-power of about 6; this tube is carried by a stout arm, which latter permits considerable angular motion of the telescope. K is a short tube, also moveable round the centre of the table; at one end of the tube is placed a photographed scale, and at the other a lens, with the scale in its principal focus.

In using this instrument, light from any source, after passing through the slit S and the plano-convex lens, instead of being allowed to fall directly on the first face of B, as in the ordinary instrument, is reflected at a right angle to its first path by means of the little prism *g*, and then passes through the prism B, suffering refraction and dispersion; the spectrum so produced is then observed with the telescope T. In accurate experiments, of course, it is necessary to measure the distances which intervene between the lines produced by any flame under examination: this is easily effected by adjusting the tube K; and when a candle is placed behind the photographed scale, a bright image of the latter is thrown into the telescope T by reflection from the second face of B. When in use, the instrument is covered with a japanned tin cap blackened inside, with slits cut in the sides to admit of

the passage of the tubes D, T, and K; by this means all extraneous light is prevented from reaching the instrument.

The point of novelty in this spectroscope is, in addition to some minor mechanical details, the reflection, by means of the prism *g*, of the bundle of rays transmitted by the slit at right angles to their former path, with a minimum loss of light and without the annoyance of secondary spectrum. The advantage gained is an important practical one. While using but one prism for dispersion, the eyepiece of the observing-telescope and the slit S are brought close together, so that the analyst, having his eye at the former, still has the slit, the gas-flame, and the platinum wire carrying the substance to be examined perfectly under his own control. The approximate position of the slit in the ordinary instruments is shown by the dotted line H, in which case it is scarcely within easy reach if the apparatus be of large size.

By thus increasing the facility with which the spectroscope may be used in the examination of minerals, I hope it may induce many mineral-chemists to employ it in their investigations, and thus render their observations on the constituents of minerals more accurate and in every way satisfactory.

I may conclude with a few hints as to the method which experience has shown me is most satisfactory in testing minerals with the spectroscope when the colourless flame of the Bunsen burner is used as the source of heat:—

1. Moisten the finely powdered mineral with pure water, take up some of the paste on a loop of platinum wire and slowly introduce into the flame, observing the spectrum produced, if any.

2. Moisten the same portion, or a fresh quantity if the first be lost, with a drop of pure hydrochloric acid; again subject the test-specimen to the heat of the flame, and observe. Most silicates yield up their bases easily by this treatment.

3. To the same test add a drop of a mixture of hydrofluoric and hydrochloric acids, allow the liquid to act for a few minutes, and then heat as before.

By adopting this systematic treatment, no substance which gives a characteristic spectrum under these circumstances can escape detection; and if the three tests be applied to the same specimen, the first one, by causing the volatilization of the greater part of the most volatile constituents, renders the succeeding stages more clear and precise.

Finally, in examining sulphides, it is necessary to heat a portion of the substance in a small closed tube; in the sulphur sublimate thallium is easily detected if it be present in the ore; and the residue needs but to be moistened with hydrochloric acid, and heated, for the detection of the presence of such metals as admit of easy recognition by the method of spectrum-analysis.

XVIII. *The Internal Motions of Gases compared with the Motions of Waves of Light.* By G. JOHNSTONE STONEY, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Dublin, July 14, 1868.

THE following pages are a collateral inquiry in a memoir on the Constitution of the Atmospheres of the Sun and Stars, which was sent to the Royal Society in May of last year. They are now separated, as it has appeared desirable to shorten that memoir. Could you spare space for them in the *Philosophical Magazine*? Their aim is to make quantitative estimates in regard to the internal motions of gases, which, though rough, may suffice for a preliminary comparison of these motions with the motions of the waves of light.

For the sake of perspicuity and to avoid wearisome periphrases, free use is made of the hypothesis that the lines of motion within gaseous molecules are or resemble orbits. All that we really know about them is that they are periodic, and that their periods are the same as those of the waves of light to which they give rise. But there is the same convenience in thus speaking in the language of a definite hypothesis, which has been provisionally adopted for the sake of this convenience, as there is when we speak of waves of light, its transverse vibrations, and the luminiferous æther, although we know from Lorenz's and Riemann's investigations that light may be an electrical phenomenon, to which the language of the wave-theory is very imperfectly applicable. And it should be distinctly kept in view that the numerical estimates at which we arrive, and which are our principal aim, are independent of this hypothesis.

The second part of the inquiry, in which the conditions which limit the extent of an atmosphere are sought out, will be found in the memoir spoken of above.

I am, Gentlemen,

Your obedient Servant,

G. JOHNSTONE STONEY.

1. The only class of bodies about the molecular constitution of which we have any satisfactory* information is gases. These

* The dynamical theory of the molecular constitution of gases, which, if I mistake not, may be ranked in point both of importance and probability along with the wave-theory of light, does not appear to have yet met with that general attention and acceptance which it seems to deserve. It may not be out of place, therefore, to add to the numberless proofs which have been drawn from its interpreting the phenomena of gases by many writers, but especially by Clausius, the following negative proof, which demon-

appear to consist of molecules moving about actively and irregularly in all directions, the path of any one being for the most part rectilinear, or, in other words, most of its motion being executed at a sufficient distance from the neighbouring molecules to be beyond the reach of sensible influence from them. Every now and then, however, each molecule comes sufficiently near some other molecule to have its course bent, on which occasions it darts off in a new direction. Moreover many facts in physics and chemistry lead irresistibly to the conclusion that the molecules are resolvable into simpler elements; and the probability distinctly is that each in most gases is a highly complex system. When a body so constituted is enclosed, the molecules by flinging themselves against the walls of the containing vessel produce the pressure of the gas. If the enclosure be at the same temperature as the gas, they do so without gain or loss of *vis viva*. But if the wall be at a higher temperature, the activity of those molecules which strike it is increased, and *vice versa*. The altered activity is shared with the rest of the gas by conduction and convection—or, more slowly, by conduction only, if the circumstances do not admit of convection; and so the temperature of the whole becomes changed.

2. If the temperature of the gas be raised while the gas is kept within a confined space, the vivacity of all the motions will be thus increased; but this will not affect d , the mean distance at which the molecules are spaced, and scarcely affect l , the average length to which a molecule can travel before it becomes engaged with another molecule. If, on the other hand, the gas

strates that no statical theory, whether on the hypothesis of a continuous substance or of distinct particles, is *possible*.

A gas is susceptible of enormous dilatation and compression without an abrupt change in the laws upon which its pressure depends; hence, if it consist of particles at rest, the force which acts in any direction on any one must be the result of forces emanating from many others, no one contributing more than a share, which may be regarded as infinitesimal. Hence it is easy to see that if the density be changed, the pressure will vary as the square of the density; for the force in any direction on any one particle will increase as the number of the particles on that or the opposite side (according as the elementary forces are attractive or repulsive) near enough to act on it, *i. e.* will increase as the density; and the number of particles subjected to this augmented force which are found within each element of volume will also have increased in the same proportion. Hence the pressure per square millimetre across any surface within the gas will increase as the square of the density; and as this is a law which does not exist in gases, it follows that no gas consists of distinct particles at rest. The same proof applies, by the principles of the Differential Calculus, to the hypothesis of a continuous and homogeneous substance. For this proof, given more at large, see Proceedings of the Royal Irish Academy, vol. vii. (1858) p. 37.

be allowed to expand without a change of temperature, d and l become larger, while the velocities are but little changed from what they were before*. If the gas be changed for another, for instance if hydrogen be substituted for oxygen at the same temperature and pressure, the number of molecules remains the same, d also remains the same, l is probably a little altered. But the mass of each molecule is now but one-sixteenth of the mass of each molecule of oxygen; and to compensate for this, the velocities with which the molecules move are 16^2 or upwards of 250 times as swift.

3. The molecular motions consist of two very distinct parts—the motions of the molecules among one another, and the motions in the interior of each molecule. In an eminently gaseous body the former of these classes of motion engrosses a large part of the *vis viva* †; but the motions are irregular, and on this account, as well as probably because they are too coarse, they are unfitted to absorb or develope vibrations in the luminous æther, except possibly in a very small degree, just as the waving of the hand may produce a slight sound. So far, therefore, as the rectilinear motions of the molecules are concerned the gas will be eminently transparent. But each molecule is also a little separate system, the constituent parts of which are in energetic motion among themselves. These minuter motions are interfered with by the neighbouring molecules only during the instants when the path of a molecule is being deflected upon its close approach to another molecule; and as the intervening times when the path is rectilinear are long in comparison with these brief instants, the internal motions of each little system will be for the most part undisturbed and therefore regular. And accordingly we must presume that it is they which influence the æther, producing those bright lines which constitute nearly the whole spectrum of an incandescent gas, and are in a corresponding degree influenced by the æther, absorbing these same rays. How wonderfully regular the internal motions of the molecules are, and at the same time how complex, appear to be

* That the velocities will be somewhat altered seems evident when we bear in mind the great change which, when a gas expands, has been effected in the ratio of the periods of perturbation to the whole time (see next section). But it seems equally plain that the alteration of the velocities will be but small except at high temperatures, and in such gases as have lines which are much dilated by the perturbations at high temperatures. This is in conformity with the comparative fixity of the two specific heats of a gas, within the limits to which experiments have been pushed.

† Nearly two-thirds in those simple gases in which the two specific heats bear to one another their usual ratio. Considerably less than two-thirds in most compound gases, in all which suffer condensation in the act of combining. See a memoir by Clausius in the *Philosophical Magazine* for 1857, vol. xiv. p. 125.

revealed to us by the fixity of the rays in the spectrum of each gas, and by their number within the limits of the visible spectrum and the doubtless greater number that exist beyond. That the motions are, however, not altogether free from perturbation is made clear by such cases as that of the lines corresponding to D in the sodium spectrum, which are not linear except at low temperatures, but of a breadth increasing with the temperature, and ill defined at the edges. This suggests a perturbation of motions that would otherwise be regular by the disturbing influence exerted by other molecules when they are near enough. If during some of the appulses the period of a vibration be somewhat accelerated, by others somewhat retarded, and if in all cases the vibration tend to settle down from the constrained towards its normal period during the undisturbed intervals between contacts, then will the effect be such as we see. If the irregularity fade away so readily as to last but a trifling part of the interval between the collisions, the line in the spectrum will be sharply defined—as in the sodium-line in a spirit-lamp, or the thallium-line in the flame of a Bunsen's burner. If it continue, though with a marked decrease of intensity, through a considerable part of the time during which the molecule is pursuing a straight course, the spectral line will have a sensible breadth and hazy edges, as in the sodium-line of a Bunsen's burner. And if the commotion be so violent that but little restoration can be effected before a fresh contact and a fresh perturbation, the spectrum of a gas may even become continuous. All the stages up to that of a continuous and nearly uniform spectrum can, for example, be produced in hydrogen. It has been observed, too, that rays of high refrangibility are more easily dilated into spectral bands of the class with which we are here dealing than rays of low refrangibility*. Perhaps this may arise from the circumstance that a ray of high refrangibility is due to an atom of the molecule the periodic time of whose motion is shorter than those of other motions which give rise to rays of low refrangibility; this atom, therefore, goes through many more of its little evolutions in the instant while a passing molecule is near enough to affect it, and is likely to suffer a proportionately more serious disturbance. If we have been right in assigning the cause of this class of dilated spectral lines, it is impossible to resist the impression that cases will sometimes arise in which the periodic time of an atom of a molecule will yield to a force tending to disturb it more freely in one direction than in the other, and will thus produce a ray expanding on one side

* This, however, is far from being uniformly the case; and there are even instances of gases, as, for example, oxygen, in which the dilatation first takes place at the red end of the spectrum.

only, or more on one side than on the other—such as is the wing on one side only of the narrowest of the magnesium lines which form the group *b* in the solar spectrum.

4. Another and quite different case which probably exists is where, in virtue of forces acting within the molecule, the periodic time of a motion within it is subject to fluctuation, going through a regular cycle of changes over and over again. In such a case, instead of a line in the spectrum we should find a band of definite breadth, and usually with well-defined edges, as in the spectra of silicon and zinc. It may be observed that this kind of fluctuation, unlike that which is attributable to a disturbing agent external to the molecule, seems quite as likely to affect motions of long as motions of short period; and accordingly the species of bands of which it is probably the cause are met with indifferently in all parts of the spectrum. Such bands are not unfrequent; and, in fact, the motions within a molecule are usually so multifarious that we often find evidence in the spectrum of a single gas of the coexistence of all the various classes of motions.

5. It is a consequence of the laws that regulate the exchange of heat, that the undulations of the æther excited by the motions of any one particular range of periods within a body can only at most appropriate to themselves a definite amount of *vis viva*, which amount is a function of θ and τ , where θ is the temperature of the body, and τ the period of vibration. Its symbolical expression would be of the form $\iint \phi(\theta\tau) d\tau dt$, where dt is the element of time, θ constant, and τ to be taken between the limits corresponding to the breadth of the spectral line or band under consideration. This puts a major limit beyond which the brightness of any particular ray of a gas at a given temperature cannot pass. But the intensities of the vibrations corresponding to the bright rays may, and generally do, in various degrees fall short of the maximum values thus fixed; or, in other words, the quantity of gas experimented on is usually too small to be absolutely opaque in reference to any of its rays, and is commonly very unequally opaque in reference to the several rays. This opacity, and consequently the energy of the orbital motions upon which it depends, is often very disproportionately affected by changes in the general *vis viva* of the system—so much so that rays which are inconspicuous as compared with others in their neighbourhood at one temperature, may become the most prominent rays at a more elevated temperature, as in iron. It sometimes happens, too, that under circumstances which have not yet been sufficiently studied, and which are but little understood, but of which temperature is one, the whole character of the motions changes—so that below a particular temperature they give rise to

one system of rays, and above that temperature to a wholly different system, as, for example, in nitrogen*.

6. When we compare different gases, we find that their molecules differ both in mass and in the motions that prevail within them. That the internal motions differ is abundantly testified by the amazing variety in the grouping of the spectral lines to which the various gases give rise. And as the number of molecules per cubic millimetre is known to be the same in all perfect gases when taken at the same temperature and pressure, it follows that the masses of the molecules are in most simple gases proportional to what chemists call their atomic weights, and, in those instances in which this is not the case, that they stand in the same simple relation to these atomic weights as the densities of the gases nearly do.

7. Very accurate measures have been effected of several of the minute magnitudes concerned in the phenomena of light; but these are the only determinations yet made in molecular physics upon which we can fully rely. Nevertheless we are not left to mere conjecture in regard to the other quantities with which we have to deal; for by many indirect methods it is possible to estimate their dimensions, not indeed with precision, but yet so as to indicate unmistakably whereabouts they lie, and, as it were, to introduce us into their neighbourhood. Such loose determinations are of great use, from their enabling us to form just conceptions of the kind of magnitudes of which we talk, and so guarding us against many errors.

8. The molecules of a gas are darting about with very different velocities; but at each temperature there is a certain mean velocity towards which the collisions that prevail within the gas tend to bring velocities which deviate from it†, and round which accordingly the innumerable velocities existing at any moment group themselves. This mean velocity Clausius succeeded in estimating at about

$$485 \sqrt{\frac{T}{273 \cdot \rho}} \text{ metres per second} \ddagger,$$

where T is the absolute temperature estimated in Centigrade degrees, and ρ the relative specific gravity of the gas compared

* The phenomena of nitrogen very explicitly declare that temperature is but one condition, and that it may be overruled by other circumstances. The lines withdrawn from the solar spectrum by the cold nitrogen of our atmosphere all (so far as they are known) belong to the nitrogen spectrum "of the second order," as it is called by Plücker—that spectrum which, in his experiments, was only obtained at the higher temperatures.

† The mean velocity so defined is not the arithmetic mean of the values of v , but the square root of the arithmetic mean of the values of v^2 .

‡ Phil. Mag. 1857, vol. xiv. p. 124.

with air. Again, Professor Maxwell has deduced from observations on the viscosity of gases, and also from the rate of diffusion of olefiant gas into air, the mean length of the path which a molecule of air describes between successive collisions. It is, at the temperature of 15° C. and the pressure of an atmosphere, about

7 eighth-metres*,

using the term eighth-metre as a convenient abbreviation of the $\frac{1}{10^8}$ th part of a metre†. This is about the ninth part of the

* The length of the mean excursions of the molecules deduced by Maxwell from a determination of the coefficient of viscosity of air which Stokes obtained from Baily's pendulum-experiments is 5·7 VIIIth-metres (Phil. Mag. 1860, vol. xix. p. 32).

From an experiment of Graham's on the rate of the diffusion of olefiant gas (a gas of nearly the same specific gravity as air) into air, he deduces the value 6·5 VIIIth-metres (Phil. Mag. 1860, vol. xx. p. 31).

Recent experiments by Maxwell himself on the viscosity of air (Phil. Trans. 1866, p. 258) assign the value 10·6 VIIIth-metres.

These determinations are very different; but even greater discrepancies would be of little moment where our object is merely to gain some insight into the order of magnitudes with which we are dealing.

The mean of the values obtained by the two methods is 7·3 VIIIth-metres.

† I here venture to employ a nomenclature for large multiples and sub-multiples which I have found so convenient that I think I may safely recommend it.

In this nomenclature the successive decimal subdivisions of the metre are :—

the decimetre,
the centimetre,
the millimetre,
the fourth-metre, IVth-metre, or IVth m.
the fifth-metre, Vth-metre, or Vth m.
and so on;

a fourth-metre meaning a metre divided by 10^4 , and so of the others. Thus microscopical quantities can be conveniently measured in fifth-metres, the Vth-metre or metre divided by 10^5 being rather more than the diameter of a corpuscle of human blood; the wave-lengths of light in VIIIth-metres, as they all lie between 39 and 77 VIIIth-metres; and so on. A fourth-metre is about the thickness of a sheet of medium writing-paper.

Again, a sixteenth-second of time is to be understood to mean a second divided by 10^{16} . This is the period in which the visible vibrations of light are to be measured—the shortest violet double vibration occupying rather more than 13 XVth-seconds, and the longest red about 25·6 XVth-seconds.

So, again, decimals may be conveniently spoken of as fifth-units, ninth-units, &c., or, as I would suggest, for euphony, and since there is so frequently occasion to speak of decimals, as fifth-eins, ninth-eins, &c., using the affix *-ein* as equivalent to *unit*. Thus the coefficient of the dilatation of platinum is 8·56 Vth-eins, that of zinc 29·4 Vth-eins, and those of most other metals between these limits. These measures are equivalent to

wave-length of the ray D. From this value and from the mean velocity* of the molecule, it follows that its path is deflected about

7 unit-nines of times in a second,

and accordingly that the mean duration of each of its little rectilinear excursions is about

14 eleventh-seconds,—

meaning by a unit-nine a unit multiplied by 10^9 , and by an eleventh-second a second of time divided by 10^{11} .

This fragment of time, tiny as it is, is nevertheless more than 50,000 times the vastly shorter period that suffices for a double vibration of red light, and than 100,000 times the duration of a double vibration of the extreme violet ray. And as the periods of all the motions within a molecule which give rise to visible

0·000,008,56 and 0·000,029,4. They may be further abbreviated into 8·56 VIth and 29·4 VIth.

A similar and equally convenient nomenclature for multiples is found in the use of affixes. Thus the decimal multiples of the metre are—

the decametre,
the hectometre,
the kilometre,
the metre-four, metre IV, or m. IV,
the metre-five, metre V, or m. V,
and so on ;

a metre-five meaning, of course, a metre multiplied by 10^5 .

Thus the mean distance of the sun is 14·74 metre-tens, which is equivalent to 147,400,000,000 metres. The distances of the fixed stars are to be measured in metre-sixteens, the distance corresponding to a parallax of 1" being 3·04 m. XVI, which means the same as 30,400,000,000,000 kilometres. Light takes about 1·06 year to travel a metre-sixteen, using Foucault's determination of the velocity of light, viz. 29·8 m. VII. (metre-sevens) per second.

So, again, using the term ohmad to designate the absolute electromagnetic unit of electrical resistance referred to the metre and second, the B.A. unit is the ohmad-seven, or ohmad multiplied by 10^7 . Resistances will be conveniently measured in ohmad-sevens or ohmad-nines, insulation in ohmad-XIII or ohmad-XV. If this nomenclature were adopted, it would keep constantly in view how the measures we may in practice find it most convenient to use are related to one another and to the absolute unit.

So, again, high numbers may be as conveniently dealt with as ordinary numbers. Thus the eye receives per second 5 unit-fourteens, or 5·XIV, of waves of yellow light. This number is the same as 5×10^{14} .

It is an advantage of this nomenclature that it is equally adapted to any language, and is equally intelligible in all.

* The mean velocity for this purpose ought to be the arithmetic mean of the values of v , which would be somewhat less than the mean given above ; but the ratio of the two means is such (nearly 11 to 12 according to Maxwell, see Phil. Mag. 1860, vol. xix. p. 23) that to use one for the other cannot prejudice an inquiry like the present, which does not admit of or attempt exactness.

spectral rays must lie between these limits, we are forced to conclude that, on the average, something like from fifty to one hundred thousand of these little* orbital revolutions are executed between two consecutive collisions. It is now no longer surprising that the temporary perturbation caused while two molecules are whirling past each other, has, in many instances, had abundant time to pass away early in the interval between two collisions, so as to leave the greater part of the minute internal motions to be executed in the undisturbed manner which the definiteness of the spectral lines attests to us. We come also to see how the motions with which the molecules dart about amongst one another cannot produce or intercept light; in fact they are far too sluggish—just as the motions of our fingers, or a very gentle waving of the hand, do not produce sound.

9. Again, Clausius has shown† that it is consistent with what we know of the most perfect gases to suppose d , the mean distance between the molecules, to be about $\frac{1}{10}$ of l , the length of

* It is very difficult, and at the same time it is of importance, because it guards us from error, to appreciate in some degree such amazingly minute intervals of time as those with which we are here concerned.

The double vibrations of visible light are executed in periods of time which range from 1·3 to 2·6 XVth-seconds. These swift little motions are accordingly related to a fifteenth-second in somewhat the same way as the motions of our limbs are to a second of time. Now the XVth-second is the same portion of a second that a second is of upwards of thirty millions of years. Hence the motions of light bear the same relation to one second of time which the motions of our limbs bear to that almost inconceivable cosmical period—a vast succession of geological ages, during which several races of animals, culminating in man, have appeared, have lasted long, and have finally perished upon our globe.

When the study of nature has guided our thoughts into such a region as this, our minds can scarce resist the impulse to wander outside the domain of science into the adjoining precincts of imagination, and to speculate that if there were sentient beings with bodies that move as deftly as this æther, and with thoughts and perceptions as quick as their bodies are active, there would be sufficient time for them within a small fragment of one second to live the lives of all the generations of men that have dwelt upon this earth, thinking all their thoughts and doing all their acts. The mind is almost carried beyond itself in the contemplation of such periods.

It is from the vastness of a second in reference to light, that with its little waves light can travel such an immense distance as 298 millions of metres in that period of time. It puts a strain upon the mind to form a conception of the mere moment of time that suffices for the light of a candle, at this unheard-of pace, to travel one hand-breadth from the flame; but brief as this instant is, it corresponds to what several weeks would be in reference to our movements: the light has had time to execute hundreds of thousands of its tiny vibrations; and it is no wonder if it has already lost all trace of everything that is fleeting or irregular in the disturbance originally created by the incandescent matter of the flame. Accordingly man has never detected any peculiarity in the vibration when examined in a situation which he calls close to its origin.

† Pogg. *Ann.* 1858, vol. iii. p. 251; or *Phil. Mag.* 1859, vol. xvii. p. 89.

the mean excursion of a molecule between its collisions; and it is, at all events, not likely that it is *much* more than $\frac{1}{60}$ in the gases experimented on by Professor Maxwell. Hence the mean distance at ordinary temperatures and pressures is probably of the same order as a ninth-metre, which is the millionth part of a millimetre. It is therefore probable that there are not fewer than something like a unit-eighteen of molecules in each cubic millimetre of a gas at ordinary temperatures and pressures*.

XIX. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period.* By JAMES CROLL, of the Geological Survey of Scotland.

[Continued from vol. xxxv. p. 384.]

IT will not do, however, to measure marine denudation by the rate at which the sea is advancing on the land. There is no relation whatever between the rate at which the sea is *advancing* on the land and the rate at which the sea is *denuding* the land. For it is evident that as the subaërial agents bring the coast down to the sea-level, all that the sea has got to do is simply to advance, or at most to remove the loose materials which may lie in its path. The amount of denudation which has been effected by the sea during past geological ages, compared with what has been effected by subaërial agency, is evidently but trifling. Denudation is not the proper function of the sea. The great denuding agents are land-ice, frost, rain, running water, chemical agency, &c. The proper work which belongs to the sea is the transporting of the loose materials carried down by the rivers, and the spreading of these out so as to form the stratified beds of future ages.

We have thus seen that geology, alike with physics, is opposed to the idea of an unlimited age to our globe. And it is perfectly plain that if there be physical reasons, as there certainly are, for limiting the age of the earth to something less than 100 millions of years, geological phenomena, when properly interpreted, do not offer any opposition.

Perhaps one of the things which has tended to mislead on this point is a false analogy which is supposed to subsist between

* Hence we may see how entirely remote from a state of emptiness that which usually passes under the name of a vacuum chamber really is. If there be a unit-eighteen of molecules in every cubic millimetre of the air about us, there will remain a unit-XV in every cubic millimetre of the best vacuums of our ordinary air-pumps. The molecules are still closely packed, within about an eighth-metre of one another; *i. e.* there are about sixty of them in a wave-length of orange light.

astronomy and geology, viz. that geology deals with unlimited *time*, as astronomy deals with unlimited *space*. A little consideration, however, will show that there is not much analogy between the two cases.

Astronomy deals with the countless worlds which lie spread out in the boundless infinity of space ; but geology deals with but one world. No doubt both reason and analogy are favourable to the idea that the age of the material universe, like its magnitude, is immeasurable—although, however, we have no reason to conclude that it is eternal, any more than we have to conclude that it is infinite. But when we compare the age of the material universe with its magnitude, we must not take the age of one of its members (say, our globe) and compare it with the size of the universe. More than this, we must not take the age of all the presently existing systems of worlds and compare their age with the magnitude of the universe ; but we must take the past history of the universe as it stretches back into the infinity of bygone *time*, and compare it with the presently existing universe as it stretches out on all sides into the infinity of *space*. For worlds precede worlds in time as worlds lie beyond worlds in space. Each world, each individual, each atom is evidently working out a final purpose, according to a plan prearranged and predetermined by the Divine Mind from all eternity. And each world, like each individual, when it serves the end for which it was called into existence, disappears to make room for others. This is the grand conception of the universe which naturally impresses itself on every thoughtful mind that has not got into confusion about those things called in science the *Laws of Nature*.

But the geologist does not pass back from world to world as they stand related to each other in the order of *succession in time*, as the astronomer passes from world to world as they stand related to each other in the order of *coexistence in space*. Besides, the researches of the geologist are not only confined to one world, but it is only a portion of the history of that one world that can come under his observation. The oldest of existing formations, so far as is yet known, the Laurentian Gneiss, are made up of the waste of previously existing rocks, and these, again, probably of the waste of rocks still prior. Regarding what succeeds these old Laurentians, geology tells us much ; but regarding what preceded, we know nothing whatever. For anything that geology shows to the contrary, the time which may have elapsed from the solidifying of the earth's crust to the deposition of the Laurentians—an absolute blank—may have been as great as the time that has elapsed since then till the present day. Physical science limits the age of the globe to a

period not exceeding something like 100 millions of years. Taking the age of the earth's crust at 100 millions of years, which probably is a high estimate, how long is it since the materials which now form these Laurentian rocks were laid down in the ancient sea-bottom in the form of sand and mud? This is a question which no one can answer; for we have no means of knowing how much of the 100 millions of years were exhausted before the Laurentian age. At all events, it must be very considerably less than 100 millions of years since the commencement of the Laurentian period.

Sir William Logan thinks that the time which separated the limestones containing the *Eozoon Canadense* from the Upper-Cambrian period may be as great as the time which elapsed between the Upper Cambrian and the nummulitic limestones of the Tertiary period. If this conjecture be anything like correct, then it is hardly possible that 50 millions of years can have elapsed since the Cambrian period. Assuming that the glacial epoch began about a quarter of a million years ago, and that the rate at which species change is uniform, we found that, adopting Sir Charles Lyell's mode of calculation, 60 millions of years have probably elapsed since the beginning of the Cambrian period. But I presume little or no weight can be placed on this mode of calculation; for it is based upon an assumption for which there is, I fear, very little warrant, viz. that the rate at which species change has been anything like uniform during geological ages. If any very great amount of time elapsed between the solidifying of the earth's crust and the Laurentian period, the probability is that the commencement of the Cambrian period does not date so far back as 60 millions of years.

Table I. shows the excentricity of the earth's orbit and longitude of the perihelion for 3 millions of years back, and 1 million of years to come, at periods fifty thousand years apart. From what we have already seen, it is very evident that 3 millions of years must stretch a considerable distance back into the geological history of our globe. And if geological climate depends upon, or is much affected by, the condition of the earth's orbit as regards excentricity, we have in this Table, combined with the other three, the means by which a rough idea of the character of the climate during at least a considerable portion of the Tertiary period may be arrived at.

TABLE I.

Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of years before and after A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.
-3,000,000	0-0365	39 30	-1,000,000	0-0151	248 22
-2,950,000	0-0170	210 39	- 950,000	0-0517	97 51
-2,900,000	0-0442	200 52	- 900,000	0-0102	135 2
-2,850,000	0-0416	0 18	- 850,000	0-0747	239 28
-2,800,000	0-0352	339 14	- 800,000	0-0132	343 49
-2,750,000	0-0326	161 22	- 750,000	0-0575	27 18
-2,700,000	0-0330	65 37	- 700,000	0-0220	208 13
-2,650,000	0 0053	318 40	- 650,000	0-0226	141 29
-2,600,000	0-0660	190 4	- 600,000	0-0417	32 34
-2,550,000	0-0167	298 34	- 550,000	0-0166	251 50
-2,500,000	0-0721	338 36	- 500,000	0-0388	193 56
-2,450,000	0-0252	109 33	- 450,000	0-0308	356 52
-2,400,000	0-0415	116 40	- 400,000	0-0170	290 7
-2,350,000	0-0281	308 23	- 350,000	0-0195	182 50
-2,300,000	0-0238	195 25	- 300,000	0-0424	23 29
-2,250,000	0-0328	141 18	- 250,000	0-0258	59 39
-2,200,000	0-0352	307 6	- 200,000	0-0569	168 18
-2,150,000	0-0183	307 5	- 150,000	0-0332	242 56
-2,100,000	0-0304	98 40	- 100,000	0-0473	316 18
-2,050,000	0-0170	334 46	- 50,000	0 0131	50 14
-2,000,000	0-0138	324 4	A.D. 1800. 0	0-0168	99 30
-1,950,000	0 0427	120 32	+ 50,000	0-0173	38 12
-1,900,000	0-0336	188 31	+ 100,000	0-0191	114 50
-1,850,000	0-0503	272 14	+ 150,000	0-0353	201 57
-1,800,000	0-0334	354 52	+ 200,000	0-0246	279 41
-1,750,000	0-0350	65 25	+ 250,000	0-0286	350 54
-1,700,000	0-0085	95 13	+ 300,000	0-0158	172 29
-1,650,000	0-0035	168 23	+ 350,000	0-0098	201 40
-1,600,000	0-0305	158 42	+ 400,000	0 0429	6 9
-1,550,000	0-0239	225 57	+ 450,000	0-0231	98 37
-1,500,000	0-0430	303 29	+ 500,000	0-0534	157 26
-1,450,000	0-0195	57 11	+ 550,000	0-0259	287 31
-1,400,000	0-0315	97 35	+ 600,000	0-0395	285 43
-1,350,000	0-0322	293 38	+ 650,000	0-0169	144 3
-1,300,000	0-0022	0 48	+ 700,000	0-0357	17 12
-1,250,000	0-0475	105 50	+ 750,000	0-0195	0 53
-1,200,000	0-0289	239 34	+ 800,000	0 0639	140 38
-1,150,000	0-0473	250 27	+ 850,000	0-0144	176 41
-1,100,000	0-0311	55 24	+ 900,000	0-0659	291 16
-1,000,000	0-0326	4 8	+ 950,000	0 0086	115 13
			+1,000,000	0-0528	57 31

On looking over Table I., it will be seen that there are three principal periods when the excentricity rose to a very high value, with a few subordinate maxima between. It will be perceived also that during each of those periods the excentricity does not remain at the same uniform value, but rises and falls, in one case twice, and in the other two cases three times. About 2,650,000 years back we have the excentricity almost at its inferior limit. It then begins to increase, and fifty thousand years afterwards, namely at 2,600,000 years ago, it reaches $\cdot 0660$; fifty thousand years after this period it has diminished to $\cdot 0167$, which is about its present value. It then begins to increase, and in another fifty thousand years, viz. at 2,500,000 years ago, it approaches to almost the superior limit, its value being then $\cdot 0721$. It then begins to diminish, and at 2,450,000 years ago it has diminished to $\cdot 0252$. These two maxima, separated by a minimum and extending over a period of 200,000 years, constitute the first great period of high excentricity. We then pass onwards for upwards of a million and a half years, and we come to the second great period. It consists of three maxima separated by two minima. The first maximum occurred at 950,000 years ago, the second or middle one at 850,000 years ago, and the third and last at 750,000 years ago—the whole extending over a period of nearly 300,000 years. Passing onwards for another million and half years, or to about 800,000 years in the future, we come to the third great period. It also consists of three maxima one hundred thousand years apart. These occur at the periods 800,000, 900,000, and 1,000,000 years to come, respectively, separated also by two minima. Those three great periods, two of them in the past and one of them in the future, included in the Table, are therefore separated from each other by an interval of upwards of 1,700,000 years.

In this Table there are three periods when the excentricity approaches to about its inferior limit, and the orbit becomes almost circular. The first is 2,650,000 years ago, when the excentricity was $\cdot 0053$; the next is at 1,300,000 years ago, when it was only $\cdot 0022$; and the next will occur in about 24,000 years hence, when its value will be $\cdot 0033$. There is, therefore, an interval of 1,350,000 years between the minima in the one case, and an interval of 1,324,000 years in the other case.

The Table shows also four subordinate periods of high excentricity, of which the one that I have supposed corresponds to the glacial epoch is the chief. Three are in the past, and one in the future.

If the glacial epoch resulted from a high state of excentricity, there must have been at least five ice-periods, including the glacial.

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cial epoch, during the past three millions of years—two as severe, and two much less severe than the glacial epoch.

It may be thought that so many as four or five ice-periods in the course of 3 millions of years past is inconsistent with the evidence of geology on that point. This, however, is at least very doubtful. It is quite possible that these three millions of years may embrace the greater part of the Tertiary period. Now we have evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch*; and it is quite possible that the evidence of more may yet be found.

But before discussing the nature of the evidence which geology affords of the existence of former glacial periods, we shall turn our attention briefly to Tables II., III., and IV. These three Tables embrace the three periods of greatest excentricity during the past 3 millions of years. The excentricity, longitude of the perihelion, &c. are given at periods of ten thousand years apart.

There are still eminent astronomers and physicists who are of opinion that the climate of the globe never could have been seriously affected by changes in the excentricity of its orbit. This opinion results, no doubt, from viewing the question as a purely astronomical one. Viewed from an astronomical standpoint, as has been already remarked, there is actually nothing from which any one could reasonably conclude with certainty whether a change of excentricity would seriously affect climate or not. By means of astronomy we ascertain the extent of the excentricity at any given period, how much the winter may exceed the summer in length (or the reverse), how much the sun's heat is increased or decreased by a decrease or an increase of distance, and so forth; but we obtain no information whatever regarding how these will actually affect climate. This must be determined wholly from physical considerations, and it is an exceedingly complicated problem. An astronomer, unless he has given special attention to the physics of the question, is just as apt to come to a wrong conclusion as any one else. The question involves certain astronomical elements; but when these are determined, everything then connected with the matter is purely physical. Nearly all the astronomical elements of the question are comprehended in the accompanying Tables.

In Tables II., III. and IV., column I. represents the dates of the periods, column II. the excentricity, column III. the

* See Lyell's 'Principles,' vol. i. chap. x. (tenth edition).

longitude of the perihelion. In Table IV. the excentricity and the longitude of the perihelion of the six periods marked with an S are copied from a letter of Mr. Stone to Sir Charles Lyell, published in the Supplement of the Phil. Mag. for June 1865; the eight periods marked L are copied from M. Leverrier's Memoir in the *Connaissance des Temps* for 1843. For the correctness of everything else, both in this Table and in the other three, I alone am responsible.

Column IV. gives the number of degrees passed over by the perihelion during each 10,000 years. From this column it will be seen how irregular is the motion of the perihelion. At four different periods it had a retrograde motion for 20,000 years. Column V. shows the number of days by which the winter exceeds the summer when the winter occurs in aphelion. Column VI. shows the intensity of the sun's heat during midwinter, when the winter occurs in aphelion, the present midwinter intensity being taken at 1000. These six columns comprehend all the astronomical part of the Tables. Regarding the correctness of the principles upon which these columns are constructed, there is no diversity of opinion. But these columns afford no direct information as to the character of the climate, or how much the temperature is increased or diminished. To find this we pass on to columns VII., VIII. and IX., calculated on physical principles. Now, unless the physical principles upon which these three columns are calculated be wholly erroneous, undoubtedly change of excentricity must very seriously affect climate. Column VII. shows how many degrees Fahrenheit the temperature is lowered by a decrease in the intensity of the sun's heat corresponding to column VI. For example, 850,000 years ago, if the winters occurred then in aphelion, the direct heat of the sun during midwinter would be only $\frac{837}{1000}$ of what it is at present at the same season of the year. Column VII. shows that this decrease in the intensity of the sun's heat would lower the temperature $45^{\circ}3$ F.

The principle upon which this result is arrived at is this:—The temperature of space, as determined by Sir John Herschel, is -239° F. M. Pouillet, by a different method, arrived at almost the same result. Taking the midwinter temperature of Great Britain at 39° , consequently, $239^{\circ} + 39^{\circ} = 278^{\circ}$ represent the number of degrees of rise due to the sun's heat at midwinter; in other words, it takes a quantity of sun-heat which we have represented by 1000 to maintain the temperature of the earth's surface at Great Britain 278° above the temperature of space. Were the sun extinguished, the temperature of our island would sink 278° below its present midwinter temperature, or to the temperature of space. But 850,000 years ago, if the

TABLE II.

I.	II.	III.	IV.	Winter occurring in aphelion.					IX.
Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of degrees passed over by the perihelion. Motion retrograde at periods marked R.	V.	VI.	VII.	VIII.	Midwinter temperature, the Gulf-stream being diminished in proportion to the excentricity.	
				Excess of winter over summer, in days.	Midwinter intensity of the sun's heat. Present intensity = 1000.	Number of degrees by which the midwinter temperature is lowered.	Midwinter temperature.		
2,650,000	0.0053	318 40	95 45			F.	F.	F.	
2,640,000	0.0173	54 25	39 12	15.4	906	26.2	12.8	6.6	
2,630,000	0.0331	93 37	33 35	22.2	884	33.3	5.7	3.3	
2,620,000	0.0479	127 12	31 24	27.4	862	38.3	0.7	— 10.5	
2,610,000	0.0591	158 36	31 28	30.6	851	41.5	— 2.5	— 15.0	
2,600,000	0.0660	190 4	30 24	30.9	850	41.8	— 2.8	— 15.4	
2,590,000	0.0666	220 28	29 28	28.3	859	39.2	— 0.2	— 11.7	
2,580,000	0.0609	249 56	27 28	22.9	878	33.9	5.1	— 4.2	
2,570,000	0.0492	277 24	27 38	16.2	902	27.1	11.9	5.3	
2,560,000	0.0350	305 2	R 6 28						
2,550,000	0.0167	298 34	R 44 36						
2,540,000	0.0192	253 58	5 21						
2,530,000	0.0369	259 19	23 48	17.1	899	28.0	11.0	4.0	
2,520,000	0.0537	283 7	26 57	25.0	871	35.9	3.1	— 7.0	
2,510,000	0.0660	310 4	28 32	30.6	851	41.5	— 2.5	— 15.0	
2,500,000	0.0721	338 36	29 0	33.5	841	44.2	— 5.2	— 18.8	
2,490,000	0.0722	7 36	28 10	33.6	841	44.3	— 5.3	— 19.0	
2,480,000	0.0662	35 46	27 40	30.8	850	41.7	— 2.7	— 15.2	
2,470,000	0.0553	63 26	25 47	25.7	868	36.6	2.4	— 8.1	
2,460,000	0.0410	89 13	20 20	19.1	892	30.0	9.0	— 1.3	
2,450,000	0.0252	109 33		11.7					

TABLE III.

1,000,000	0.0151	248 22	65 28	15.3	906	26.1	12.9	6.7
990,000	0.0224	313 50	44 12	20.5	887	31.5	7.5	-0.4
980,000	0.0329	358 2	34 38	22.8	878	33.8	5.2	-4.1
970,000	0.0441	32 40	34 9	24.0	874	35.0	4.0	-5.8
960,000	0.0491	66 49	31 2	23.0	878	34.0	5.0	-4.3
950,000	0.0517	97 51	29 51	19.7	890	30.6	8.4	0.4
940,000	0.0495	127 42	28 29	14.2	910	25.0	14.0	8.2
930,000	0.0423	156 11	25 29					
920,000	0.0305	181 40	12 35					
910,000	0.0156	194 15	R 59 13					
900,000	0.0102	135 2	R 8 1					
890,000	0.0285	127 1	25 32	21.2	884	32.2	6.8	-2.0
880,000	0.0156	132 33	27 50	28.2	859	39.0	0.0	-11.5
870,000	0.0607	180 23	29 18	32.9	843	43.6	-4.6	-18.0
860,000	0.0708	209 41	29 47	34.7	837	45.3	-6.3	-20.3
850,000	0.0747	239 28	29 46	32.4	845	43.2	-4.2	-17.4
840,000	0.0698	269 14	29 14	29.0	857	40.0	-1.0	-12.8
830,000	0.0623	298 28	27 36	22.1	881	33.1	5.9	-3.1
820,000	0.0476	326 4	22 26					
810,000	0.0296	348 30	R 4 41					
800,000	0.0132	343 49	R 50 30					
790,000	0.0171	293 19	10 18	15.2	907	26.0	13.0	6.7
780,000	0.0325	303 37	25 1	21.2	884	32.2	6.8	-2.0
770,000	0.0455	328 38	28 34	25.1	870	36.0	3.0	-7.2
760,000	0.0540	357 12	30 6	26.7	864	37.7	1.3	-9.5
750,000	0.0575	27 18	31 12	26.1	867	37.0	2.0	-8.6
740,000	0.0561	58 30	32 25	23.6	876	34.6	4.4	-5.2
730,000	0.0507	90 55	34 19	19.6	890	30.6	8.4	0.4
720,000	0.0122	125 14	52 12	14.3	910	25.0	14.0	8.2
710,000	0.0307	177 26	30 47					
700,000	0.0220	208 13						

TABLE IV.

I.	II.	III.	IV.	Winter occurring in aphelion.				
Number of years before A.D. 1800.	Excentricity of orbit.	Longitude of perihelion.	Number of degrees passed over by the perihelion. Motion retrograde at periods marked R.	V.	VI.	VII.	VIII.	IX.
				Excess of winter over summer, in days.	Midwinter intensity of the sun's heat. Present intensity = 1000.	Number of degrees by which the midwinter temperature is lowered.	Midwinter temperature.	Midwinter temperature, the Gulf-stream being diminished in proportion to the excentricity.
250,000	0.0258	59 39	15 19	17.4	898	F. 28.3	F. 10.7	F. 3.7
240,000	0.0374	74 58	27 51	22.2	885	33.2	5.8	-3.2
S 230,000	0.0477	102 49	21 44	23.2	877	34.1	4.9	-4.6
S 220,000	0.0497	124 33	20 22	26.7	864	37.7	1.3	-9.5
S 210,000	0.0575	144 55	23 23	26.5	865	37.4	1.6	-9.1
200,000	0.0569	168 18	21 46	24.7	871	35.7	3.3	-6.7
S 190,000	0.0532	190 4	19 18	22.1	881	33.1	5.9	-3.1
S 180,000	0.0476	209 22	18 45	20.3	887	31.3	7.7	-0.6
S 170,000	0.0437	228 7	8 31	16.9	900	27.8	11.2	4.3
160,000	0.0361	236 38	6 18	15.4	905	26.2	12.8	6.5
150,000	0.0332	242 56	3 33	16.1	903	26.9	12.1	5.6
140,000	0.0346	246 29	13 5	17.8	896	28.8	10.2	3.0
130,000	0.0384	259 34	15 13	20.1	888	31.0	8.0	-0.1
120,000	0.0431	274 47	19 1	21.4	883	32.4	6.6	-2.1
110,000	0.0460	293 48	22 30	22.0	881	33.0	6.0	-2.9
100,000	0.0473	316 18	23 44	21.0	885	32.0	7.0	-1.5
L 90,000	0.0452	340 2	24 11	18.5	894	29.4	9.6	2.1
L 80,000	0.0398	4 13	23 9	14.7	908	25.5	13.5	7.5
L 70,000	0.0316	27 22	18 46					
L 60,000	0.0218	46 8	4 6					
50,000	0.0131	50 14	R 21 38					
L 40,000	0.0109	28 36	R 22 46					
L 30,000	0.0151	5 50	38 10					
L 20,000	0.0188	44 0	34 28					
L 10,000	0.0187	78 28	21 2					
0	0.0168	99 30						

winters occurred in aphelion, the heat of the sun at midwinter would only equal 837 instead of 1000 as at present. Consequently, if it takes 1000 parts of heat to maintain the temperature 278° above the temperature of space, 837 parts of heat will only be able to maintain the temperature $232^{\circ}\cdot7$ above the temperature of space; for $232^{\circ}\cdot7$ is to 278 as 837 is to 1000. Therefore, if the temperature was then only $232^{\circ}\cdot7$ above that of space, it would be $45^{\circ}\cdot3$ below what it is at present.

This method of calculating how much the temperature is lowered by a given reduction of the sun's heat is that given by Sir John Herschel in his 'Outlines of Astronomy,' § 369 *a*. About three years ago, in an article in 'The Reader,' I endeavoured to show that this method is not rigidly correct. It results from the principles of the dynamical theory of heat, and is also supported by experiments made by Professor Draper, of New York, and others, that the rate at which a body radiates its heat off into space is not directly proportionate to its absolute temperature. The rate at which a body loses its heat as its temperature rises, increases more rapidly than the temperature. As a body rises in temperature, the rate at which it radiates off its heat increases; but the *rate* of this increase is not uniform, but increases with the temperature; consequently the temperature is not lowered in proportion to the decrease of the sun's heat. But this error is probably neutralized by one of an opposite nature, to which I shall now refer.

We know that absolute zero is at least 493° below the melting-point of ice; consequently, if the heat derived from the stars is able to maintain a temperature of -239° or 222° of absolute temperature, then nearly as much heat is derived from the stars as from the sun. But if so, why do the stars give so much heat and so very little light? If the radiation from the stars could maintain a thermometer 222° above absolute zero, then space must be far more transparent to heat-rays than to light-rays, or else the stars give out a great amount of heat but very little light, neither of which suppositions is probably true. The probability is, I venture to presume, that the temperature of space is not much above absolute zero. In this case, by adopting -239° as the temperature of space, we make the values given in column VII. too small. But as the two errors tend to neutralize each other, these values may in the meantime be accepted as not very far from the truth, or at least as near as can be arrived at in the present state of science on this point. But whether these values be too high or too low, one thing is certain, that a very slight increase or a very slight decrease in the quantity of heat received from the sun must affect temperature to

a considerable extent. The direct heat of the moon, for example, cannot be detected by the finest instruments which we possess; yet from 238,000 observations made at Prague during 1840-66, it would seem that the temperature is sensibly affected by the mere change in the lunar perigee and inclination of the moon's orbit*.

Column VIII. gives the midwinter temperature. It is found by subtracting the numbers in column VII. from 39° , the midwinter temperature. Column IX. shows the midwinter temperature of the centre of Scotland, on the supposition that the Gulf-stream was diminished in volume in proportion to the excentricity. In former papers it was explained how a change of excentricity must affect ocean-currents†.

I have not given a Table showing the temperature of the summers at the corresponding periods. This could not well be done; for there is no relation at the periods in question between the intensity of the sun's heat and the temperature of the summers. One is apt to suppose, without due consideration, that the summers ought to be then as much warmer than they are at present, as the winters were then colder than now. Sir Charles Lyell, in his 'Principles,' has given a column of summer temperatures calculated upon this principle. Astronomically the principle is correct, but physically it is totally erroneous, and calculated to convey a wrong impression regarding the whole subject of geological climate. The summers at those periods, instead of being much warmer than they are at present, would in reality be far colder than they are now, notwithstanding the great increase in the intensity of the sun's heat resulting from the diminished distance of the sun. If a country is free from snow and ice, then no doubt the temperature will rise during summer as the intensity of the sun's heat increases; but when such a country is enveloped in perpetual snow and ice, the temperature of the summers will never rise much above the freezing-point, no matter what the intensity of the sun's heat may be. The physical reason of this was explained on a former occasion‡. In a country covered with ice, the direct heat of the sun is often very intense, in fact scorching. It will raise the temperature of the mercury in the thermometer exposed to the direct rays of the sun, but it fails to heat the air. Captain Scoresby, for example, when in lat. $80^{\circ} 19' N.$, found the side of his ship heated by the direct rays of the sun to about 100° ,

* See Professor C. V. Zenger's paper "On the Periodic Change of Climate caused by the Moon," *Phil. Mag.* for June 1868.

† *Phil. Mag.* for August 1864 and February 1867.

‡ *Phil. Mag.* for February 1867.

while the air surrounding the ship was actually 18° below the freezing-point. On another occasion he found the pitch melting on the one side of the ship by the heat of the sun, while water was freezing on the other side from the intense coldness of the air.

The mean temperature of Van Rensselaer Harbour, in lat. $78^{\circ} 37'$ N., long. $70^{\circ} 53'$ W., was accurately determined from hourly observations made day and night over a period of two years by Dr. Kane. It was found to be as follows:—

Winter	. .	$-28^{\circ}59$
Spring.	. .	$-10^{\circ}59$
Summer	. .	$+33^{\circ}38$
Autumn	. .	$-4^{\circ}03$

But although the quantity of heat received from the sun at that latitude ought to have been greater during the summer than in England*, yet, nevertheless, the temperature is only $1^{\circ}38$ above the freezing-point.

The temperature of Port Bowen, lat. $73^{\circ} 14'$ N., was found to be as follows:—

Winter	. .	$-25^{\circ}09$
Spring.	. .	$-5^{\circ}77$
Summer	. .	$+34^{\circ}40$
Autumn	. .	$+10^{\circ}58$

Here the summer is only $2^{\circ}4$ above the freezing-point.

If we go to the Antarctic regions, where the influence of ice is still more felt, we find the summers even still colder. Capt. Sir James Ross found, when between lat. 60° and 77° S., that the mean temperature never rose even to the freezing-point during the entire southern summer; and when near the ice-barrier on the 8th of February, 1841, a season of the year equivalent to August in England, he had the thermometer at 12° at noon, and so rapidly was the young ice forming around the ships that it was with difficulty that he escaped being frozen in for the winter. And on the February of the following year, when he again visited that place, he had the thermometer standing at 19° at noon, and the sea covered with an unbroken sheet of young ice as far as the eye could reach from the mast-head. This extraordinary low temperature at that season of the year was wholly owing to the presence of the ice. Had there been no ice on the Antarctic continent, Sir James would have had a summer

* See Mr. Meech's memoir "On the Intensity of the Sun's Heat and Light," Smithsonian Contributions, vol. ix.

hotter than that of England, instead of one actually below the freezing-point.

Now, during the glacial epoch, when Europe was almost covered with snow and ice, the summers could not possibly have been much warmer than they are at present in Arctic and Antarctic regions. In other words, during the glacial epoch the mean summer temperature would be very little above the freezing-point.

[To be continued.]

XX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 68.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read :—

“On the Impact of Compressible Bodies, considered with reference to the Theory of Pressure.” By R. Moon, M.A., Honorary Fellow of Queen’s College, Cambridge.

Suppose that we have two *rigid* cylinders of equal dimensions, which have their axes in the same straight line; suppose, also, that one of the cylinders is at rest while the other moves towards the first with the velocity V in a direction parallel to both the axes; the consequence of the collision which under such circumstances must take place, will manifestly be that half the momentum of the moving cylinder will be withdrawn from it, and will be transferred to the cylinder which originally was at rest.

The mode in which velocity or momentum will thus be collected from the different parts of the one cylinder, and distributed amongst those of the other, is obvious. Exactly the same amount will be withdrawn from the velocity of each particle of the impinging cylinder, and exactly the same amount of velocity will be impressed on each particle of the cylinder struck.

And the reason of this is equally obvious, since, if such were not the case, the particles of each cylinder would *contract*—a supposition which is forbidden by the very definition of rigidity.

But if, instead of being perfectly rigid, each cylinder is in the slightest degree compressible, a variation in the effect will occur.

As before, momentum of finite amount will be transferred from the one cylinder to the other, but the mode of collection of the velocity withdrawn from the one, and the mode of distribution of that injected into the other, will no longer be the same as before.

In order that the moving cylinder may not be reduced to absolute rest by the collision, it is obvious that the cylinder originally at rest, or a portion of it, must be moved out of the way, so as to allow of the continuance, even in a modified degree, of the other’s

motion; and this can only be effected on the terms of a transference of velocity or momentum taking place from the one cylinder, or part of it, to the other cylinder, or part of it.

But when the cylinders are compressible, we are freed from two conditions which obtain when the cylinders are rigid.

In the first place, it is no longer necessary to suppose, neither should we be justified in assuming, that the velocity abstracted from each particle of the impinging cylinder, or transferred to each particle of the cylinder struck, is the same; on the contrary, all experience tells us that, in bodies susceptible of compression, compression is always produced by collision—in other words, that variation of velocity, in the parts about which the collision takes place, is the immediate and invariable concomitant of collision.

In the second place, when the cylinders are compressible, it is no longer essential to suppose that the effect of the collision will be to withdraw velocity from every particle of the impinging cylinder, and to impart velocity to every particle of the cylinder struck. Undoubtedly such may be the case if the cylinders are short, if they are possessed of only a moderate degree of rigidity, and if the velocity before impact of the impinging cylinder is considerable. But if the cylinders be long, while the velocity of the impinging cylinder is of moderate amount, the contrary may occur. The condition that the cylinder originally at rest shall not oppose an immediate insurmountable barrier even to the modified motion of the other may, obviously, be sufficiently satisfied if a motion of contraction is imparted by the collision to a definite portion of the second cylinder.

But when the cylinders are compressible, equally as when they are rigid, the collision must cause the instantaneous abstraction of velocity or momentum, either from the whole of the impinging cylinder, or from a definite part of it, and the instantaneous communication of the velocity so withdrawn, either to the whole of the cylinder struck, or to a definite part of it.

We have hitherto assumed the velocity of each particle of the impinging cylinder to have been originally uniform. Let us now suppose, however, that immediately before impact a counter velocity of variable amount is impressed on the different parts of the impinging body, so that, at the instant of impact, *before taking account of the effect of collision*, the velocity at any point of the impinging body may be expressed by $V - V_1$,—where V is constant, but V_1 has the value zero at the surface of collision, and thence gradually increases as we recede towards the other extremity of the cylinder, so that $V - V_1$, which expresses the velocity of the impinging cylinder before impact, has its greatest value at the surface of collision, and diminishes as we recede therefrom.

It is clear that, in the case we are now considering, the collective momentum abstracted from the impinging cylinder by the collision will be less, and finitely less, than that which was abstracted by the collision in the former case, in which the velocity of each particle of the impinging cylinder was supposed uniform and equal to V .

For, if M be the momentum lost by collision when the velocity before impact is uniform and equal to V , it is clear that when the velocity before impact is represented by $V - V_1$, the quantity V_1 may be such that the momentum *before impact* may be finitely less than M ; from which it follows inevitably that the amount of momentum lost by collision in this latter case must be less than M .

Let us now vary the *data* by supposing that the velocity before impact *increases* instead of diminishes as we recede from the surface of collision, so that at the moment of impact, before taking account of the effects of collision, the velocity at any point of the impinging cylinder is represented by $V + V_1$ instead of $V - V_1$.

It is clear that the momentum abstracted by the collision in this latter case will be *greater*, and finitely greater, than in the case where the velocity before impact is uniform and equal to V . Let the additional momentum abstracted in this case be M_1 , the whole momentum so abstracted being represented by $M + M_1$.

Let us now make a final variation in the conditions of the problem, by supposing that at the moment of impact, and irrespective of the impact, a velocity equal and opposite to V is communicated to each particle of the impinging cylinder, so that at that instant, without taking account of any action of the one cylinder upon the other, the velocities of the two cylinders along their surfaces of contact will be equal, or, rather, will be alike zero, at the same time that at every other point of the impinging cylinder there will be a variable velocity V_1 increasing in amount as we recede from the surface of contact.

In estimating the effect of the cylinders being in contact under the circumstances last described, it is clear that the abstraction from each particle of the impinging body of the velocity V can only be regarded as preventing the transference to the second cylinder of so much of the momentum $M + M_1$ as that velocity, if it had constituted the entire velocity before impact of the impinging body, would have given rise to, viz. M ,—and that the momentum M_1 , whose appearance in the expression $M + M_1$ is due to the fact of the first cylinder having been originally endowed with the variable velocity V_1 in addition to the constant velocity V , will still continue to be transmitted to the second cylinder from the first.

We are thus led to this singular and, doubtless, pregnant conclusion, that in a continuous material system in which there is neither discontinuity of velocity nor discontinuity of density, all the consequences of collision may occur, viz. the instantaneous transmission of a finite amount of momentum from one part of the system to another, provided we have discontinuity in the *tendency to compression* in the different parts of the system.

The author has endeavoured, in former communications to the Royal Society, to show that when the velocity in a fluid diminishes in the direction to which the motion tends, the slower particles will offer a resistance to the motion of the faster particles, which the received theory fails to take into account. The foregoing speculation

goes to prove that the circumstance of the surfaces of contact of contiguous elements of the fluid having the same velocity constitutes no objection to the reality of such resistance.

GEOLOGICAL SOCIETY.

[Continued from p. 74.]

April 15th, 1868.—Sir R. I. Murchison, Bart., K.C.B., F.R.S., &c.,
Vice-President, in the Chair.

The following communications were read :—

1. "On the Disposition of Iron in variegated Strata." By George Maw, Esq., F.G.S.

In this paper the author gave detailed descriptions of the phenomena of variegation in rocks of various ages and composition, with numerous analyses of the differently coloured portions. The conclusions at which he had arrived were the following:—(1) That the assumed production of the colouring-matter of red beds from the decomposition of Iron Pyrites appears untenable. (2) That the bleaching of red beds by the reducing action of fossil carbonaceous matter will not account for the facts of variegation. (3) That this bleaching and nearly every form of variegation of red beds are caused by the passage of iron from the discoloured areas, unaccompanied by any change of combination, excepting the invariable conversion of the anhydrous into the hydrated peroxide. (4) That the arrangement of the iron has not been produced by solution. (5) That it has in some cases been produced by a variety of evident conditions independent of mere chemical reaction, while in other cases it seems to have occurred arbitrarily. (6) That it is unaccompanied by any other change in the combination or position of the constituents of the stratum, and appears to be wholly independent of its chemical constitution, any simple chemical theory being, in short, insufficient to account for the movement of the iron. (7) That the motion has sometimes taken place centripetally and sometimes centrifugally. (8) That the partial conversion of the carbonate of protoxide of iron into hydrated peroxide is one of the most frequent purely chemical causes of variegation. Other causes are the conversion of the red anhydrous into the yellow hydrated peroxide, the secondary formation of the bisulphide and the several stages of its decomposition in mechanical association with the peroxide, and, lastly, the infiltration of lime and magnesia into red beds. (9) That some of the more complicated forms are due partly to segregation, and partly to changes of combination. (10) That the ferruginous banding of yellow sandstones is the result of the segregation of the hydrous peroxide of iron into lines, which are invariably adjacent to a bleached part of the stratum, over which they appear to have advanced, gathering up the peroxide of iron in their course, and leaving behind them a depleted area.

2. "On the older Rocks of South Devon and East Cornwall." By Harvey B. Holl, M.D., F.G.S.

The author divided the rocks of the district to which the communication referred into a Lower, Middle, and Upper South-Devon Group, and stated that the lowest beds were brought up along a line of country extending from Dartmoor by Kingston Down to the Brown Willey granite, where they formed a broad anticlinal axis. These rocks are unfossiliferous, and may not be lower in the series than the base of the Ilfracombe group of North Devon, or the highest part of the group immediately below it, the latter being more probably represented by some still lower beds of red and greenish grits brought up to the surface in the anticlinal axis of St Broeck's Down further to the west.

The Middle South-Devon Group comprises at its base the discontinuous calcareous range of the Looe River, St. Germans, Brickfortleigh, Ashburton, and Bickerton, above which is a mass of blue and claret-coloured slates, which separate it from the Plymouth or Torbay Range.

This calcareous and fossiliferous group is succeeded by higher beds of blue and claret-coloured argillaceous slates, followed by hard, red, dark, micaceous schists, and purple and greenish grits, which constitute the author's Upper South-Devon Group. These rocks are very sparingly fossiliferous, and probably correspond to the upper and Morthoe portions of the Ilfracombe series of North Devon.

The unconformable position of the Culm-measures is seen in the circumstance that they rest upon different parts of the underlying Devonian rocks—sometimes on the limestones of the Torbay range, sometimes on the slates, at others on the volcanic rocks. This entirely separates the South-Devon rocks from the Carboniferous system.

The occurrence of the genus *Pteraspis*, and probably *Cephalaspis* with *Phyllolepis concentricus* and (?) *Holoptychius*, and other fish-remains, appeared to the author to go a good way towards identifying these Cornish and South-Devon beds with the Old Red Sandstone of Scotland. These fossils range up to the very base of the Torbay limestones.

The author referred the whole of the rocks treated of, with the exception of the purple and greenish grits of St. Broeck's Down, to the Middle Devonian system, and considered that if the lower or Linton rocks were to be met with at all on the south side of the Culm-trough, it would be in the high ground which forms the watershed of West Cornwall.

In the concluding portion of the paper Dr. Holl entered upon the palæontological relations of the different South-Devon groups, and especially those of the Petherwyn beds.

XXI. *Intelligence and Miscellaneous Articles.*

ON SECONDARY CURRENTS AND THEIR APPLICATIONS.

BY M. G. PLANTÉ.

OBSERVATIONS of the intensity of the secondary current developed in a voltameter with lead electrodes induced me, in 1860, to construct a *pile* or *secondary battery* by which, within certain limits, the work of the voltaic battery could be accumulated*. I have now to describe a new mode of arranging the apparatus, as well as the effects of *quantity* and of *tension* which can be obtained by its means.

The instrument, which I designate *secondary quantity battery*, consists of a rectangular gutta-percha vessel provided with a number of lateral grooves very close to each other, and containing a number of lead plates immersed in water acidulated by sulphuric acid. The odd-numbered plates are all joined, and may be connected with one of the poles of a battery; the even plates are also joined, and may be connected with the other pole, so that all the plates form one single secondary element of large surface.

When, on such an apparatus constructed with six lead plates 20 centims. square, the current of two small nitric-acid couples is allowed to act, and when the circuit is broken by means of the commutator so as to close the secondary one, temporary incandescence is obtained of a platinum wire 1 millim. in thickness and 8 centims. in length—a result which would otherwise be impossible with the feeble source which furnishes the principal current.

To obtain more powerful effects, I have constructed batteries of from twenty to forty lead plates united together; these batteries produce very intense calorific effects, such as the fusion of iron or steel rods, when they have been charged by two or three Bunsen's elements from 15 to 20 centims. in height.

In the production of many of the effects of voltaic electricity, the *quantity* of electricity furnished by the current would be insufficient if it were not associated with a certain *tension*. M. Thomsen of Copenhagen described, in 1865, a *polarization battery* based on the current produced by platinized platinum electrodes, and by means of which he obtained a series of secondary currents sufficiently close to form a continuous series having higher tension than the principal one.

I thought that the intense current furnished by lead electrodes might be used for producing temporary effects of great tension, and I have attained this object by arranging the secondary couples so that they could be charged simultaneously in *quantity* and then discharged in *tension*.

The *secondary tension battery* which I have constructed consists of forty secondary couples, each formed of a very narrow gutta-percha vessel containing two lead plates, 20 by 20, immersed in dilute acid. The poles of all the couples contained in the same box terminate in a commutator so constructed that it can join them either in surface or in tension.

If this battery be charged by the aid of three Bunsen's elements

* Phil. Mag. S. 4. vol. xix. p. 468.

of mean size, and the commutator be so turned as to close the secondary circuit, a platinum wire more than 2 metres long and $\frac{1}{4}$ millim. thick can be made incandescent for a few instants. Chemical decompositions requiring great tension may also be shown, as well as physiological effects and a voltaic arc of short duration. In fact, we obtain for a brief space the same effects as are produced by a battery of fifty-five to sixty elements arranged in a series.

The calorific effects produced by this apparatus might be used for firing mines, and the physiological effects for medical purposes.

In short, the secondary battery with lead electrodes enables us to obtain either powerful temporary effects of quantity from a continuous source of electricity of small quantity, or powerful temporary effects of tension from a continuous source of small tension.

This concentration of the work of the battery is doubtless only obtained through the intervention of a chemical action, and is effected with the losses inevitable in every organ of change. There is not, as in the case of induction for instance, the direct production of one physical effect by another physical action; but the final result is none the less an accumulation or a modification of electrical force which can be utilized in certain circumstances.

These facts show, moreover, the importance of the part which secondary currents must play in electrochemistry, and the applications which may result therefrom.—*Comptes Rendus*, June 22, 1868.

ON THE FORMATION OF PEROXIDE OF SILVER BY OZONE.

BY F. WÖHLER.

If, through water feebly acidulated by sulphuric acid, the current from a few Bunsen's elements be passed, and if a silver plate be used as the positive electrode, it immediately begins to be covered with a black substance. This substance is peroxide of silver, easily recognizable by its property of dissolving in ammonia with a brisk evolution of nitrogen. Thus prepared it forms black amorphous crusts, unlike the beautifully crystallizing peroxide which separates from a dissolved silver-salt at the positive pole.

This mode of formation is interesting, inasmuch as it probably takes place directly from the ozone at the positive pole; for it was long ago observed by Schönbein, and noted as one of the characteristic properties of ozone, that bright silver is superficially changed into peroxide by ozonized oxygen. In the experiments the current was strong enough to form ozone when platinum was used as an electrode; but when silver was used no trace of any odour of ozone could be perceived; and it must therefore be assumed that all the ozone is used for the oxidation of the silver.

After a certain quantity of peroxide has been deposited on the silver, it begins slowly to disengage oxygen; and simultaneously there is deposited on the negative pole a small quantity of grey amorphous metallic silver. At the same time there is always a small quantity of silver in solution. These are probably secondary phenomena, arising from the action of the sulphuric acid collected at the positive pole, which decomposes the peroxide.—*Liebig's Annalen*, May 1868.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1868.

XXII. *On the Polarization of Heat at 100° C., and on the Motion accompanying Heat-conduction.* By PROFESSOR MAGNUS*.

IN a former essay on the polarization of heat, and on its passage through parallel plates†, it has been already shown that the heat radiated from a red-hot plate of platinum at an oblique angle only comes in part from the surface, another portion being radiated from the interior. This fact was deduced as a consequence of the polarization of the heat radiated from such a surface. For since the plane of polarization has the same position as that of light refracted at a certain angle, we are compelled to assume that, at all events, one portion of the emergent rays suffers a refraction at the surface; and in order that this may be the case, the heat must come from the interior of the plate. Since, however, this polarization takes place according to the same laws as that of light, we are further compelled to conclude that the propagation of the heat in the interior takes place in the same manner as that of light, namely in transverse oscillations. In the above-mentioned publication it was asserted that the conduction of heat depends upon this species of motion. This assertion, however, was only based upon the fact that the motion which is called heat cannot be of two kinds, and that, if its propagation through air, vacuum, or any other diathermanous substance takes place by means of transverse oscillations, its propagation in the interior of such bodies as are not diathermanous, which propagation we call conduction, must be of the same nature.

This conclusion could not be drawn with certainty, because it

* *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin.* März 1868.

† *Monatsbericht*, 1866, p. 62.

was still possible that it was only the luminous portion of the heat which was polarizable. If, on the contrary, we could prove that the heat which is radiated at an oblique angle from bodies of any temperature, and therefore at a very low one, is also partly polarized, it would be proved that in the case of dark bodies the heat which they radiate comes in part from their interiors, and is propagated in them by transverse undulations. I think, then, we should be justified in asserting that the conduction of heat, or its propagation in athermanous bodies, also depends upon transverse oscillations.

MM. La Provostaye and Desains state that they obtained polarized heat from a platinum plate whose temperature was below incandescence. Their experiments were made with platinized plates at a temperature of between 330° and 360° C.; and with these they say they have obtained very distinct polarization, though to a less degree than with smooth plates. It was shown in the above-mentioned experiments with incandescent plates that these do not show any polarization if they are perfectly platinized. The plates used by MM. Provostaye and Desains, therefore, could not have been fully platinized. These experimenters do not enter into any explanation of the polarization of the heat radiated from these plates. It is also clear that by the employment of rough plates they were not in a position to arrive at the conclusion that the polarization depended upon the refraction of the heat issuing from the interior. For the same reason, their results, although very interesting, determine nothing in regard to the point at issue. It was therefore necessary to undertake new experiments, in order to decide whether the heat which is radiated from bodies of a lower temperature (say, 100° C.) is polarized.

The means hitherto employed for the investigation of the polarization of heat depend upon its passage through double-refracting plates or through columns of mica plates. Neither of these could be here employed, because they do not allow the passage of the dark heat. There only remained, therefore, to make use of reflection for this purpose. Since, however, as is well known, only a small portion of the heat falling upon a mirror is reflected, especial precautions and arrangements had to be made in order to measure the reflected heat.

The essential portion of the apparatus was a reflecting mirror, 65 millims. wide and 130 millims. long, of black polished glass. This was placed at the end of a horizontal tube of 60 millims. diameter and 120 millims. length. This tube was made of stout pasteboard, in order to prevent heat reaching the mirror by conduction. Both ends of the tube were closed by plates having circular openings of 35 millims. diameter. The axis of the tube, or the line joining the centres of the openings, passed, when produced, through the centre of the mirror, which could receive any

inclination required towards the axis. In the following experiments the two always made an angle of 35° with one another. The thermo-electric pile with its cone was so fastened to the mirror by means of an arm, that the rays falling upon the mirror in the direction of the axis of the tube were reflected into the cone of the pile. The entire arrangement could be turned with the tube and its axis so that the mirror and thermo-pile could be brought into all azimuths. To protect them from external influences, they were surrounded by a large pasteboard box, which was further protected by thick hanging covers. Only a small part of the tube extended beyond the box, and by this means the mirror and thermo-pile could be turned from the outside without opening the box. An index fastened to the tube showed the position in the interior. Between the box and the source of heat another large screen was erected, in which there was an opening of the same size as those at the two ends of the tube. As the rays passed through these three openings, they reached the mirror in almost parallel directions. For the source of heat a tin-plate vessel was employed, which could be maintained at 100° C. by means of a current of steam. This steam-box was so fastened upon a stout wooden slab that it could be turned upon a horizontal axis whose continuation passed through the centre of the mirror. The face of this vessel, which radiated upon the mirror, made an angle of 35° with the horizontal. Other plates could be fastened upon the steam-box, so as to serve as radiating surfaces. The steam entered the vessel out of a large glass bolt-head, and left it by means of a tube which led into a large flask containing many litres of cold water. In order to remove the water condensed in the vessel which served as the source of heat, the tube which led off the steam was so bent in the inside of the box, that its opening always reached down to the lowest part. When, therefore, the water had reached this point, it was forced out of the box by the freshly arriving steam. A moveable screen of double metal plates stood between the radiating vessel and the fixed screen, in which the first diaphragm was situated. It was removed whenever an observation had to be made, and replaced as soon as the needle had completed its deviation.

The observation was made by observing the first displacement of the needle. It may, it is true, appear that it would be preferable to wait until the needle had assumed a constant position. But the results are then less sharp and exact. During the long continuance of the radiation the pile becomes, in fact, warmed, and consequently the position of the needle never remains quite constant. At the same time the space in the inside of the box becomes warmed, and the same value is not always obtained on repeating the same experiment. Accordingly the method of

observing the first deviations was retained, since, as appears from the numbers given below, the values obtained on repeating the same experiment agree as closely with one another as can be expected in such experiments. The vessel serving as source of heat was, in the first place, so turned that the radiating surface was vertical and turned to one side (say, to the left). It was then turned horizontally towards the ceiling, then vertically towards the other side, and finally horizontally towards the floor. For each of these positions the magnitude of the deviation was determined by turning the reflecting mirror in succession vertical towards one side, then horizontal towards the ceiling, and vertical towards the other side. The construction of the apparatus did not allow of its being turned horizontally downwards.

For each of these positions the observation was repeated three times, and the mean of these observations, taken immediately one after the other, was adopted. When the radiating surface is directed upwards or downwards, we should have expected equal values for similar positions of the mirror. Nevertheless, in spite of all care to avoid the introduction of errors through variations in the adjustment, the values obtained were invariably higher when the radiating surface was turned upwards than when it was directed downwards. The cause of this is evidently the superior warmth of the roof of the room in these experiments above the floor. The former was, in fact, warmed by the air ascending from the radiating vessel and from the neighbouring bolt-head of boiling water. The roof accordingly radiated more heat than the floor or the walls of the room, which was not artificially heated; and a portion of this heat, reflected by the radiating surface itself, reached the pile along with that directly radiated. Hence also the fact is accounted for, that the difference found on turning the radiating surface upwards and downwards was greatest when the radiating surface was of polished metal; for in such surfaces the radiating-power is very small, while the reflection is greater than from any others. Since through the wintry cold during which these experiments were conducted the window-side and the back of the room had sometimes different temperatures, large screens were set up to diminish this injurious influence, at half a metre on both sides of the radiating vessel. In observations such as these, indeed, the greatest care is requisite. The galvanometer used was the very sensitive one constructed for former experiments, and since improved.

If the heat radiated at 100° C. had not been polarized, the values must have been the same at every position of the radiant surface for each of the positions mentioned of the reflecting surface. The numbers given below, however, prove that the deflections of the galvanometer were smaller at every position of the radiant surface when its plane of reflection coincided with that of the

mirror, than when the two formed a right angle with one another. Hence it follows that the radiant heat is partly polarized, and that, as with incandescant platinum plates, its plane of polarization is perpendicular to the plane of reflection. Starting from the hypothesis that the deflections of the galvanometer are proportional to the quantities of heat, the values found give the relation between the polarized and the entire amount of radiated heat. For, assuming that the mirror in both positions reflects only polarized heat, the entire radiated heat is proportional to the sum of the reflected quantities in the two positions of the mirror, and the difference of these quantities is proportional to the polarized portion.

In the first place, experiments were made in which the polished face of the radiating vessel made of tinned iron-plate sent its heat against the reflecting mirror. In the following Table are contained three series of experimental numbers obtained in this manner. In the first the original observations are given, in order to show how far these agree with the mean values; in the remainder the mean values alone are given. A polished copper plate was then fastened upon the tin plate, and this again was afterwards replaced by a polished plate of aluminium. The latter was used to decide whether metals of very different specific gravity might show differences in the polarization, and consequently in their refractive powers.

TABLE I.—Radiating surface :—polished metal.

Radiating surface.	Analyzing mirror.	Tinned-iron plate.							Copper.	Aluminium.	
		Series of experiments I.				II.	III.	I.		II.	
					mean.	mean.	mean.	mean.	mean.	mean.	
Horizontal upwards.	Left.....	9	8.5	9	8.75	7.4	9	9.2	7.3	7.6	
	Horizontal ...	4.5	5	5	4.8	3.7	6.5	6.8	4	4.1	
	Right	8	8.5	9	8.5	7.25	9.2	8.7	7.2	6.6	
	Left.....	3	3.75	4	3.5	3	2.8	1.6	2.25	
To the left.	Horizontal ...	5	5	5	5	3.4	5.5	3.1	3.3	4.5	
	Right	4.5	5	4.25	4.5	2.1	3.6	2.6	2.1	2.9	
	Left.....	6.25	6	6.5	6.25	5.2	3.1	4.5	4.3	
	Horizontal ...	4	4	4	4	2.75	3	2.1	2.8	2.7	
Horizontal downwards.	Right	5.5	6	5.75	5.75	5.2	6	3.75	4.9	4.6	
	Left.....	3.25	3	3.5	3.25	4.5	2.6	2.6	2.8	3.2	
	Horizontal ...	5	4.5	5	4.8	4.6	5	3.1	4	3.9	
	Right	4.25	3.25	3.25	3.6	2.3	2.8	2.75	3.1	2.8	
Mean of all values with perpendicular planes of reflection						6.46	5.57	6.65	5.16	5.2	5.3
Mean of all values with parallel planes of reflection						3.96	3.07	3.58	3.27	2.7	3
Total heat or sum						10.42	8.64	10.23	8.43	7.9	8.3
Polarized portion or difference						2.50	2.50	3.07	1.89	2.3	2.3
Percentage of polarized portion of the entire heat						24	29	30	22.4	29	28

Upon the radiating surface of the tinned-iron vessel a transparent glass plate of 1 millim. thickness was now fastened. It is clear that the temperature assumed by the outer surface of this glass plate was lower than that of the metallic face upon which it was fastened, and from which it derived its heat. Nevertheless the quantities of heat which it delivered are much greater than those radiated from the metal plate. This superior action of the glass cannot be due to its sending out heat from its entire mass; for it is known from Melloni's researches that glass does not allow heat of 100° C. to pass through. Consequently the superiority of the glass must be entirely owing to its greater radiating-power.

TABLE II.—Radiating surface :—transparent glass.

Radiating surface.	Analyzing mirror.	Series of experiments I.				II.	III.
					mean.	mean.	mean.
Horizontal upwards.	Left.....	44	43·5	43·25	43·6	39·3	47·25
	Horizontal ...	35	35	34·75	34·9	33	36·25
	Right	42	42·5	42	42·2	42·7	45·25
To the left.	Left.....	35	35·25	35	35·1	30·2	36·3
	Horizontal ...	42	42·5	42	42·2	36·5	45·3
	Right	34·5	34·25	34·5	34·4	32·7	35·2
Horizontal downwards.	Left.....	44	43·75	44	43·9	39·5	45·41
	Horizontal ...	35·5	35·5	35	35·3	33·5	36·8
	Right	43	43	43	43	42·2	43·5
To the right.	Left.....	35·25	36	34·5	35·6	31·3	35·7
	Horizontal ...	42·75	42	42	42·2	37·1	43
	Right	39·5	35	35·5	35·5	31·8	35·7
Mean of all values with perpendicular surfaces of reflection					42·8	39·5	44·95
Mean of all values with parallel surfaces of reflection.					35·1	32·1	35·99
Total heat or sum.....					77·9	71·6	80·94
Polarized portion or difference					7·7	7·4	8·96
Percentage of polarized portion					9·9	10·3	11·1

The numbers show that the heat radiated from the glass at an angle of 35° is also polarized, and indeed perpendicularly to the plane of reflection, exactly as with metals. In order, however, to get rid entirely of the notion that radiation had taken place through the substance, the transparent plate was replaced by one made of black perfectly opaque glass. With this the following values were obtained :—

TABLE III.—Radiating surface :—black glass.

Radiating surface.	Analyzing mirror.	Series of experiments I.				II.	III.
					mean.	mean.	mean.
Horizontal upwards.	Left	44.5	46.25	45	45.2	36	42.1
	Horizontal ...	34.7	35	35	35	27.7	34.7
	Right	44	44.5	44.5	44.3	33.8	45
To the left.	Left	34	34	33	33.7	28	34.5
	Horizontal ...	44	43.5	44.25	44	34.5	42
	Right	34.75	35	34	34.6	26	35.7
Horizontal downwards.	Left	45	45	45	45	35.5	42
	Horizontal ...	34	35	34	34.3	27.5	35.5
	Right	44.5	45.5	44	44.7	33.6	45.2
To the right.	Left	35	35.5	36.5	35.6	27.5	33.7
	Horizontal ...	44	43.5	44.25	44	34.2	42.3
	Right	34.75	34.5	35	34.75	26.8	36.3
Mean of all values with perpendicular planes of reflection					44.8	34.6	43.1
Mean of all values with parallel planes of reflection					34.6	27.2	35.1
Total heat or sum					79.4	61.8	78.2
Polarized portion or difference					10.2	7.4	8.0
Percentage of polarized portion					12.9	12.0	10.3

The agreement of these values with those given by the transparent plate is a new proof that the transparent glass is no more diathermanous for heat of 100° C. than the black opaque glass, and that both kinds of glass possess radiating-powers for heat which are very nearly equal.

If the polarization of the radiated heat depended upon the partial emergence of the heat from the interior and its refraction on leaving the surface, then with a perfectly rough surface the refraction must take place in all directions, and consequently no polarization at all should be observable*. It appeared, therefore, of interest to try how the polarization was affected by using a dull, ground glass plate in place of the polished one. The rough glass plate was of the same kind of glass and of the same thickness as the smooth one before used. As is seen from the following numbers, the polarization was much less than with the smooth plate; but still polarization did occur. Hence the suggestion immediately offered itself that the glass was not sufficiently rough. Accordingly a perfectly rough substance, namely

* "Wenn die Polarisation der ausgestrahlten Wärme davon herrührt, dass die Strahlen zum Theil aus dem Innern kommen und beim Austreten an der Oberfläche gebrochen werden, so müsste für den Fall, dass die Oberfläche vollkommen rauh wäre, die Brechung nach allen Richtungen stattfinden und daher gar keine Polarisation zu beobachten sein." As the above appears ambiguous, I give the text literally.—F. G.

black cloth, was chosen, and stuck for this purpose upon a tin plate and fastened upon the radiating vessel.

The cloth, as is shown by the following numbers, exhibited no polarization at all; and herein lies a further proof that the radiant heat comes in part out of the interior of the body, and is polarized at its surface by refraction.

TABLE IV.—Radiating surfaces :—rough glass and black cloth.

Radiating surface.	Analyzing mirror.	Rough glass.		Black cloth.	
		I.	II.	I.	II.
		mean.	mean.	mean.	mean.
Horizontal upwards...	Left.....	40·2	44·8	41·5	42·5
	Horizontal	35·3	38·7	41·7	41·5
	Right	38·3	42·3	39·8	41·5
To the left.	Left.....	34·8	38·1	39·7	42·9
	Horizontal	37·5	42·6	39·7	41·8
	Right	34·5	37·1	39·2	41·4
Horizontal downwards	Left.....	38·8	43	40	43·4
	Horizontal	34·5	37·4	39·2	41·8
	Right	38·7	41·6	40	42·3
To the right.	Left.....	34·3	37·3	40·5	42·25
	Horizontal	38	42·2	38·8	41
	Right	34·2	38	40	41·4
Mean of all values with perpendicular planes of reflection...		38·6	42·7	39·97	42·1
Mean of all values with parallel planes of reflection		34·6	37·7	40·05	41·9
Total heat		73·2	80·4	80·02	84·0
Polarized portion		4·0	5·0	1·92	0·2
Percentage of polarized portion...		5·5	6·2		

The propagation in the interior of a body to the surface may be regarded as radiation; but then we must admit that such a radiation takes place in all parts of the inside of athermanous bodies. It is in fact known that the metals, such as gold and silver, are diathermanous in very thin sheets. This has been shown by M. Knoblauch for solar heat. For heat of 100° C. and lower temperatures, such as here employed, no observations had been previously made.

It follows from this investigation, that when heat is propagated in the interior of bodies, transverse oscillations take place at every temperature. Or if such oscillations be not linear, they are yet such motions as to have components perpendicular to the direction of propagation, which produce the same effect as rays of heat.

XXIII. *On the General Solution of Algebraic Equations.*

By THOMAS P. KIRKMAN, M.A., F.R.S.*

ALL algebraic equations can be solved as clearly as the cubic and the quartic, that of the $(n+1)$ th degree after that of the n th, by the use of the following theorem, and of familiar algebraic operations.

Let any of the various ways of writing the group Ω of Πn substitutions made with the n elements $1, 2, 3, \dots, (n-1), n$, as a product of $n-1$ groups, be this,

$$\Omega = G_2 \cdot G_3 \cdot G_4 \dots G_{n-2} \cdot G_{n-1} \cdot G_n,$$

where G_r is a group of the r th order, in which the $n-r$ final elements are undisturbed.

Let H_1 be any given asymmetric function of the n variables $x_1, x_2, x_3, \dots, x_n$ which has Πn values by their permutations.

Let $G_n \cdot H^i$ be the sum of the n values of H_1^i made by the substitutions of G_n , and let

$$G_n^a \cdot H^i = J_i^a$$

be the a th power of that sum. It is here convenient to write the exponent a in an algebraical sense over G_n ; and it can obscure nothing, since G_n^2 , considered as the group product $G_n \cdot G_n$, is simply G_n .

Also let

$$G_{n-1}^b \cdot G_n^a \cdot H^i = G_{n-1}^b \cdot J_i^a$$

be the b th power of the sum of $n-1$ values of J_i^a which are made on the group G_{n-1} ; and so on.

And let Γ_m be the group of the m cyclical permutations of the first m elements, the remaining $n-m$ being undisturbed.

We can construct without any difficulty, except tedious algebraic routine, the function following, for any given system of positive exponents that we desire,

$\Sigma_\beta = \Gamma_n Z_\gamma^\beta = \Gamma_n \Gamma_{n-1}^\beta \Gamma_{n-2}^\gamma \dots \Gamma_5^\lambda \Gamma_4^\mu G_2^p G_3^m G_4^l \dots G_{n-2}^c G_{n-1}^b G_n^a \cdot H^i$,
which is the sum of n given values of the β th power of

$$Z_\gamma = \Gamma_{n-1} Y_\delta^\gamma,$$

which is the sum of $n-1$ given values of the γ th power of

$$Y_\delta = \Gamma_{n-2} X_\epsilon^\delta \text{ \&c.,}$$

till we come to read the function

$$\Gamma_4 S_n^p,$$

* Communicated by the Author.

the sum of four given values of the p th power of

$$S_m = G_2 R_l^m,$$

the sum of two given values of the m th power of

$$R_l = G_3 Q_k^l,$$

the sum of three given values of the l th power of Q_k , and thus finally to read the function

$$J_i = G H^i,$$

the sum of the n values of the asymmetric H_1^i which we choose to construct on G_n .

Our theorem is:

(1) The first sum of values so read, but the last in construction, viz.

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta,$$

is a given rational and symmetric function of the n variables x_1, x_2, \dots, x_n , whatever be the positive exponents $\beta, \gamma, \dots, b, a, i$.

(2) Every one of the succeeding sums of values, viz.

$$\Gamma_{n-1} Y_\delta^\gamma, \Gamma_{n-2} X_\epsilon^\delta, \dots, \Gamma_4 S_m^p, G_2 R_l^m \dots G_n H^i,$$

is a given irrational and symmetric function of those variables, if the solution of equations of the n th and inferior degrees is known.

The demonstration is very simple.

(1) It is evident that

$$S_m = R_l^m + {}_1R_l^m = (Q_k^l + {}_1Q_k^l + {}_2Q_k^l)^m + ({}_3Q_k^l + {}_4Q_k^l + {}_5Q_k^l)^m$$

is invariable by Γ_2, Γ_3 , which represents all the six permutations of x_1, x_2, x_3 . It follows that

$$\Gamma_4 S_m, \text{ or } \Gamma_4^\mu S_m^p,$$

whatever be the integers μ and p , is a function invariable by the $\Pi 4$ permutations of x_1, x_2, x_3, x_4 ; whence of necessity

$$\Gamma_5^\lambda \Gamma_4^\mu S_m^p$$

is invariable by the $\Pi 5$ permutations of x_1, x_2, x_3, x_4, x_5 ; and so on, till we see that

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta$$

is invariable by the permutations of $x_1, x_2, x_3, \dots, x_n$; *i. e.* the function Σ_β is rational and symmetrical in these n variables, whatever be the positive exponents.

It will be here useful to make a distinction between the roots a_1, a_2, \dots, a_n of the equation

$$(x-a_1)(x-a_2)(x-a_3)\dots(x-a_{n-1})(x-a_n)=0=U,$$

or the *entering roots* of $U=0$ (as I call them), all asymmetric

functions of the n elements a_1, a_2, \dots, a_n , and the roots

$$\{a_1\}, \{a_2\}, \{a_3\}, \dots \{a_n\},$$

which we obtain by solution of $U=0$: these I call the *issuing roots* of $U=0$. They are all irrational functions symmetric in the n elements a_1, a_2, \dots, a_n , or in any set of n elements of which Σa^r is a symmetric function.

I denote by a bracketed function $\{R\}$ always a symmetric irrational function of our variables x_1, x_2, \dots, x_n , the issuing root of a given equation which we are supposed to have solved.

(2) As our rational symmetric function Σ_β can be formed, and is supposed to be formed, for every system of exponents that we require, we can write for any system in which β only is variable the n equations

$$\begin{aligned} Z_\gamma + {}_1Z_\gamma + {}_2Z_\gamma + \dots + {}_{n-1}Z_\gamma &= \Sigma_1, \\ Z_\gamma^2 + {}_1Z_\gamma^2 + {}_2Z_\gamma^2 + \dots + {}_{n-1}Z_\gamma^2 &= \Sigma_2, \\ \vdots &\vdots \\ Z_\gamma^n + {}_1Z_\gamma^n + {}_2Z_\gamma^n + \dots + {}_{n-1}Z_\gamma^n &= \Sigma_n, \end{aligned}$$

where $Z^r, {}_1Z^r, {}_2Z^r, \dots, {}_{n-1}Z^r$ are n values of Z^r made on Γ_n . From these n conditions we obtain

$$\{Z_\gamma\} = \Gamma_{n-1}Y_\delta^\gamma,$$

the issuing root of a given equation of the n th degree, having for coefficients rational and symmetric functions of x_1, x_2, \dots, x_n . This symmetric and irrational function $\{Z_\gamma\}$ can be written for every system of exponents $\gamma, \delta, \dots, c, b, a, i$, in which γ only is variable; that is, we have

$$\begin{aligned} Y_\delta + {}_1Y_\delta + {}_2Y_\delta + \dots + {}_{n-2}Y_\delta &= \{Z_1\}, \\ Y_\delta^2 + {}_1Y_\delta^2 + {}_2Y_\delta^2 + \dots + {}_{n-2}Y_\delta^2 &= \{Z_2\}, \\ \vdots &\vdots \\ Y_\delta^{n-1} + {}_1Y_\delta^{n-1} + {}_2Y_\delta^{n-1} + \dots + {}_{n-2}Y_\delta^{n-1} &= \{Z_{n-1}\}. \end{aligned}$$

Hence

$$\{Y_\delta\} = \Gamma_{n-2}X_\epsilon^\delta$$

is the issuing root of a given equation of the $(n-1)$ th degree, whose coefficients are irrational and symmetric functions of our n variables. And by the same certain process which has transformed the asymmetric sums $\Gamma_{n-1}Y_\delta^\gamma$ and $\Gamma_{n-2}X_\epsilon^\delta$ into the symmetric irrational functions $\{Z_\gamma\}$ and $\{Y_\delta\}$, we can go on through the entire list of asymmetric sums of values for every system of exponents, transforming each sum into the issuing root of a given equation, till finally we obtain the system

$$\begin{aligned} H_1 + H_2 + H_3 + \dots + H_n &= \{J_1\} \\ H_1^2 + H_2^2 + H_3^2 + \dots + H_n^2 &= \{J_2\}, \\ \vdots & \\ H_1^n + H_2^n + H_3^n + \dots + H_n^n &= \{J_n\}; \end{aligned}$$

and the n asymmetric values of H_1 made on G_n are thus finally transformed, by the solution of a given equation of the n th degree, into the irrational shapes

$$\{H_1\}, \{H_2\}, \{H_3\}, \dots \{H_n\},$$

which are all symmetric functions of x_1, x_2, \dots, x_n .

From this position to the solution of the general equation $\Theta=0$ of the $(n+1)$ th degree, there is but one, little, easy step.

If α be any imaginary root of $y^{n+1}=1$, and if $x_0, x_1, x_2, \dots, x_n$ be the roots of $\Theta=0$, we have

$$x_0 = \Sigma x + (\alpha + \alpha^2 + \alpha^3 + \dots + \alpha^{1+n})(x_1 + x_2 + x_3 + \dots + x_n);$$

or, which is the same thing,

$$x_0 = \frac{1}{n+1} \Sigma x + \frac{1}{n+1} P_1^{\frac{n+1}{n+1}} + \frac{1}{n+1} P_2^{\frac{n+1}{n+1}} + \dots + \frac{1}{n+1} P_n^{\frac{n+1}{n+1}},$$

where

$$\begin{aligned} P_1 &= x_0 + \alpha x_1 + \alpha^2 x_2 + \alpha^3 x_3 + \dots + \alpha^n x_n, \\ P_2 &= x_0 + \alpha x_2 + \alpha^2 x_3 + \alpha^3 x_4 + \dots + \alpha^n x_1, \\ P_3 &= x_0 + \alpha x_3 + \alpha^2 x_4 + \alpha^3 x_5 + \dots + \alpha^n x_2, \\ &\vdots \\ P_n &= x_0 + \alpha x_n + \alpha^2 x_1 + \alpha^3 x_2 + \dots + \alpha^n x_{n-1}, \end{aligned}$$

$P_2, P_3, P_4, \dots, P_n$ being here formed with P_1 on $G_n = \Gamma_n$ by the cyclical permutations of x_1, x_2, \dots, x_n , while x_0 and α, α^2, \dots are undisturbed.

This expression of x_0 becomes, after raising P_1, P_2, \dots, P_n each to the $(n+1)$ th power by ordinary involution,

$$\begin{aligned} x_0 &= \frac{1}{n+1} \Sigma x + \frac{1}{n+1} (S + H_1)^{\frac{1}{n+1}} + \frac{1}{n+1} (S + H_2)^{\frac{1}{n+1}} \\ &\quad + \dots + \frac{1}{n+1} (S + H_n)^{\frac{1}{n+1}}, \end{aligned}$$

where S is a symmetric function of $x_0, x_1, x_2, \dots, x_n$, and therefore a rational function of the coefficients of $\Theta=0$, and where H_1 is a function of the same $n+1$ variables which has $n!$ values by their permutations. And since H_1 is invariable by the cyclical permutations of $x_0, x_1, x_2, \dots, x_n$, because $P^{n+1} = (\alpha^n P)^{n+1}$, what-

ever r may be, it receives all its values by the permutations of the n variables $x_1, x_2, x_3, \dots, x_n$.

Wherefore, by what precedes, the n values of H_1 ,

$$H_1, H_2, H_3, \dots, H_n,$$

can be transformed into the issuing roots

$$\{H_1\}, \{H_2\}, \{H_3\}, \dots, \{H_n\},$$

which are symmetric irrational functions of $x_1, x_2, x_3, \dots, x_n$, and also of necessity symmetrical irrational functions of the $n+1$ variables x_0, x_1, \dots, x_n . These functions $\{H\}$ are consequently transformable into given irrational functions of the coefficients of $\Theta=0$, and the algebraic solution of $\Theta=0$ is infallibly completed by the substitution of these issuing roots so transformed, for the n asymmetric functions H_1, H_2, \dots, H_n in our value of x_0 . All this can be verified for the simple cases of $n+1=3$, $n+1=4$, and $n+1=5$.

It may be supposed that these processes will be encumbered with hopeless ambiguity by the multiplicity of values of the issuing roots

$$\{Z_r\}, \{Y_r\}, \{X_r\}, \dots, \{J_r\},$$

which appear in the coefficients of the equations of the n th and lower degrees which have to be solved.

But the truth is that there is no ambiguity at all, if we take care that the issuing roots

$$\{Z_1\}, \{Z_2\}, \dots, \{Z_{n-1}\}$$

are all corresponding roots (that is, if we read them all with the same radicals affected by the same signs and imaginaries), and if we take care that the issuing roots

$$\{Y_1\}, \{Y_2\}, \dots, \{Y_{n-2}\},$$

$$\vdots$$

as well as

$$\{J_1\}, \{J_2\}, \dots, \{J_n\},$$

are in each set of solutions a set of corresponding roots. For the rest, it matters not which of the n issuing roots we take for $\{Z_r\}$, nor, after that, which of $n-1$ issuing roots we take for $\{Y_s\}$ &c. By whatever path we choose to march, we shall assuredly arrive at the symmetrical expression of $\{J_i\}$ the sum of n values of H_i^1 , formed either on G_n , or on one of its $\Pi(n-1)-1$ derivatives; and x_0 is correct if expressed in n values of the H_1 above found, which are formed on any one of those derivatives. In other words, we may write for P_1, P_2, \dots, P_n in our expression of x_0 the n results of operation on them by any substitution, not in G_n , made with the n elements $1, 2, 3, \dots, n$.

We have, in our expression of the values of H , taken G_n identical with Γ_n , which is a course by no means necessary. It simplifies our view of the group Ω of Πn substitutions to suppose G_m everywhere identical with Γ_m ; and if Ω be constructed even in this unskilful way, the theorem will support us and bring us safely to our goal.

No selection of a form for Ω can save us from the necessity of constructing the symmetric function Σ_β for every combination of the positive exponents a, b, c, \dots, l, m, p that satisfies

$$a < n+1, b < n, c < n-1, \dots l < 5, m < 4, p < 3;$$

but the Greek exponents need not always all be positive, as for certain systems Σ_β is symmetrical when one or more or all of them are zero, which reduces the Γ carrying the zero to a factor unity. The best choice of form for Ω will introduce in Σ_β the greatest number of zeros among the Greek exponents, and thus cause an enormous reduction of the algebraic labour. This labour will be abridged by the computer in proportion to his command of the theory of groups.

I have thus the pleasure of accomplishing a reasonable number of demonstrated impossibilities, mingled with regret at the ruin of one of the most charming of ancient mysteries. It is a comfort to know that no true Briton for the next twenty years will so far sin against all that is dignified and venerable in mathematics as to suffer himself either to understand or to believe one word of this. To the scientific reader who thinks of taking some day a second peep at this paper, I would say—don't; you will only step out of that enchanted fog of analytical sublimity into a boundless and commonplace bog of algebraic inutility. There is nothing here which adds to my right or to my wish to detain the philosopher. I am quietly satisfied, but far from being elated, by these results. In the opinion of myself and of my respected friend Posterity (the only section of the scientific public in these practical islands whose mathematical reading will ever qualify them to form anything like a judgment on the precise points of comparison), this finishing contribution to the rich theory of equations is a trifle compared with my extensive additions to the skeleton theory of groups, which are again a trifle compared with my creation of the entire theory of the polyedra. How sagaciously amused our profundities will find themselves, if ever they hear of these burrowings of a country mouse!

Croft, near Warrington.

XXIV. *On certain Objections to the Theory of the Equivalent of Refraction.* By Dr. ALBRECHT SCHRAUF, *Custodian of the Imperial Mineralogical Cabinet of Vienna*.*.

ON a cursory examination of the optical literature of the past, we find numerous essays, of various dates, which, being based upon the theory of emission, have denied the correctness of the hypothesis of vibration. The idea of refractive power introduced into optics by Newton appears to have the same purport (*Schicksal*). Besides these older investigations, the most recent work of Rühlmann† are intended to show the inapplicability and incorrectness of the refractive power. Since in the above-named investigation several of my previously published conclusions on this subject are declared to be erroneous, I take this opportunity of placing these objections in their proper light.

I imagine, moreover, that there are many assertions in M. Rühlmann's criticism which he would not have published if he had not ignored, for reasons unknown to me, my later investigations—both those on “Equivalents of Refraction”‡ (1865), and my “Studies”§ published in December 1866. These two investigations, compared with my former publications (1862), contain many new points of view, and precisely in that very field which M. Rühlmann has chosen as the starting-point of the discussion. It appears, indeed, that the doubts now published by M. Rühlmann concerning the theory of refractive power were considered and explained by me in the course of the preceding year.

If we examine the results of M. Rühlmann's important work on the Thermo-optical Constants of Water, the first point which demands some attention is the sentence on page 202||, “That the magnitude M given by Schrauf is by no means a constant, but continually varies with the temperature.”

The variation of the refractive power with the temperature, however, is not unknown to me, and received my mature consideration long ago. In my “Studies,” p. 69, I stated a year previously, “A uniform result of the calculation, however, is that the values of M (refractive power) diminish with increasing temperature. The obvious objection here arises, that, in consequence of this observation, the function

* Translated by Frederick Guthrie, F.R.S.E., from a separate impression, communicated by the author, from Poggendorff's *Annalen*, vol. cxxxiii. p. 479.

† Poggendorff's *Annalen*, vol. cxxxii. p. 193.

‡ Schrauf, “Die Refraktionsäquivalente und optischen Atomzahlen der Grundstoffe,” *Sitzungsb. der Wien. Akad.* 1865, vol. lii. (2) p. 176.

§ Schrauf, *Physikalische Studien über die gesetzmässigen Beziehungen von Materie und Licht*. Wien, Gerold, 1867, p. 249.

|| Pogg. *Ann.* vol. cxxxii. Phil. Mag. S. 4. vol. xxxv. p. 395.

wandte Physik der Krystalle*, I have in Chap. XIX. § 131, discussed the action of conducted heat upon light.

The developments carried out on pp. 352–354 are based essentially on the following fundamental propositions.

On the accession of free external heat, an alteration takes place in the length of the molecular paths and the rate at which they are traversed; hence an alteration in the volume and temperature. The changes in these two (length and rate), however, do not take place symmetrically, and they may have the same or opposite signs. In the latter case a contraction may take place on increasing the temperature of a body, or an expansion on cooling it.

We are taught by Fizeau's observations that in the case of certain solid bodies the indices of refraction increase even when the temperature increases. My calculations of the refractive power of liquid bodies show that the refractive indices diminish more rapidly than is due to the alteration in volume indicated by the temperature.

For the explanation of this fact we must assume that the amount of heat entering the body has a twofold effect. The change of volume corresponds to a change in the index of refraction, which corresponds to the alteration in density (formula 3). An increase of the temperature (that is, an alteration in the mean molecular velocity), will, by increasing the molecular velocity†, cause a diminution in the usual retardation of the luminous impulse in those cases where (as in gases and liquids) the molecules are in unstable equilibrium. In support of this is the fact that, according to my calculations in liquid bodies, the refractive power diminishes with an increase of temperature. This action of the temperature on the retarding force appears to be proportional to the variations of temperature, and the light-retarding forces X_v of the substance. (Compare my *Lehrbuch der Physik der Krystalle*.)

In contradistinction to gases and liquids, the molecules of solid bodies have no free motion, but are bound together in constant equilibrium. An elevation of temperature and increase of molecular velocity cannot, therefore, in all cases be regarded as assisting the work and motion performed by the rays of light.

Although, on the one hand, in part an increase and in part a decrease of the light-retarding forces X_v of the solid body may correspond to an increase of temperature, still, on the other hand, from the fact that in Iceland spar the one index of refrac-

* Schrauf, *Lehrbuch der physikalischen Mineralogie*, vol. ii. *Lehrbuch der angewandten Physik der Krystalle*. Wien, Braumüller, 1868, p. 426.

† Compare Schrauf, *Physik der Krystalle*, p. 145. The propagation of the wave impulse.

tion diminishes, while the other increases with elevation of temperature, it follows that the influence of heat is considerably influenced by the internal grouping of the atoms around the axis.

Although, however, this secondary influence of temperature has long been known to me, I do not feel on this account in the least compelled to regard the theory of refractive power as incorrect. The following are, amongst others, my reasons:—

In my “Studies” (p. 115) I have said, “Many hypotheses are possible with regard to the variations of the magnitudes $Z G X_v$. The first is that the forces X_v are dependent upon the chemical properties. More attentive consideration, however, shows that there can be no physical reason why a little mass of O should act differently upon the light to an equal mass of H. We are hence led to the hypothesis that the retarding forces X_v are equal for different elements—an assumption which is rendered more than probable by the results obtained by it.”

This hypothesis may be retained for the first approximation, which suffices in exactness for all hitherto performed investigations, even when we allow that X_v is a function of the temperature. The abnormal variation of the refractive power M remains within very narrow limits if all possible errors are eliminated. It is further to be remarked that we are always at liberty, by dint of comparing the refractive equivalents of various substances, to limit the constancy of X_v to the ordinary temperatures of observation. This I did in my “Studies.”

Such comparison of the retarding forces, X_v , is possible under all circumstances, and must take place under exactly the same points of view under which the temperatures of the different bodies are themselves measured. Even for the latter we are accustomed to consider as equal the temperatures, and therefore, implicitly, the molecular-path velocities of various bodies in different thermal states, although we know little about the law of increase of temperature in bodies and the expenditure of free heat necessary to effect it.

In comparing the optical relations of various bodies, we can therefore still use the theory of the refractive equivalent, inasmuch as for this theory the proportion

$$\frac{X(a)}{X_v(b)}$$

is a constant for the same temperature.

If, on the other hand, we compare the refractive indices of a body for different temperatures, the twofold action of the influx of conducted heat is clearly distinguishable. The overpowering

action of the change of volume declares itself in the law of the refractive power; the change in the molecular path has, on the other hand, a secondary, very insignificant positive or negative influence upon the function X_v of the luminous propagation.

The proof of these statements is given by the numerous data in my "Studies." However, I may here collect the most important facts to prove that, by comparing different bodies for equal temperatures, even of the most diverse volumes, we may look upon the retarding forces X_v as constant, or even as equal. According to formula (II.), the constancy of M proves the constancy of X_v .

		D.	μ .	M.	N ² .
a. Carbon *.....	Anthracite	1.4	1.720	0.001809	
	Diamond	3.5	2.434	0.001820	
b. Water	Ice	0.9181	1.3089	0.0010046	
	Water	1.0	1.3330	0.0010048	
c. Titanic acid...	Anatase	3.95	mean 2.499	mean 0.0017167	
	Rutile	4.20	2.560	0.0017098	
d. Carbonate of calcium ...	Iceland spar	2.73	mean 1.5801	0.0007185	
	Arragonite	2.94	1.6178	0.0007115	
e. C ³ H ⁶ O ²	Propionic acid.....	0.996	1.376	0.001160	0.01266
	Formate of ethyle ...	0.940	1.357	0.001157	0.01271
f. C ⁷ H ¹⁴ O ²	Enanthylic acid.....	0.917	1.409	0.001391	0.01584
	Acetate of amyle.....	0.863	1.390	0.001398	0.01559
g. C ² H ⁴ O, 2(C ² H ⁴ O)	Aldehyde	0.781	1.322	0.001239	0.01416
	Butyric acid	0.961	1.387	0.001243	0.01376
h. C ⁵ H ¹⁰ O 2(C ⁵ H ¹⁰ O)	Valeral.....	0.799	1.377	0.001451	0.01672
	Valerianate of amyle.	0.858	1.400	0.001450	0.01630

These examples will certainly amply suffice to prove that we may consider the function X_v as constant as soon as the bodies are compared at the same temperature. Examples *g* and *h* even show that, for the polymers of optically identical elements, X_v is not merely a constant but has the same value.

In regard to the polymers of optical *allomeric* elements, I must refer to my "Studies"†.

In view of the examples given, it may fairly be doubted whether new analytical formulæ are necessary to establish the relation between density and the indices of refraction.

Moreover I did not satisfy myself with the above-mentioned facts alone before I decided upon the assertion that the retarding forces X_v always bear the character of constants for equal temperatures. A further proof, for instance, is possible, based upon formula II. If we multiply both sides of this equation (II.) with the chemical equivalent P , we obtain on the left the Newtonian

* For the calculation of M , the density of air is taken = 1.

† Compare Schrauf, 'Studies,' p. 215; Schrauf, *Physik der Krystalle*, p. 310.

is based upon the data of Biot and Arago, which are to the effect that the variation of the indices of refraction is proportional to the increase of density. They say, "*La réfraction d'un même gaz quelconque est toujours rigoureusement proportionnelle à sa densité*"*. Biot and Arago nevertheless decide to retain the Newtonian refractive power, since for gases, as is well known,

$$\mu^2 - 1 = 2(\mu - 1),$$

and hence the investigations on gases do not make manifest any distinction between formulæ (II.) and (VI.).

Beer, in his 'Introduction to Higher Optics' (p. 36), has again brought forward this formula (VI.). In a similar manner it has been recently employed by Dale and Gladstone, Landolt and Haagen.

In my 'Equivalents of Refraction' (1865) I have submitted this formula to an examination. To distinguish it from Newton's refractive power, I called formula (VI.) Biot's refractive power, and denoted it by the letter m . In the same manner I determined for a great number of substances, not only the Newtonian refraction-equivalents \mathfrak{N} , but also those values corresponding to the formula

$$P\left(\frac{\mu-1}{D}\right) = Pm = m. \quad . \quad . \quad . \quad (VII.)$$

These values may also claim to be considered refraction-equivalents. I distinguish them by the name "Biot's refraction-equivalents" (formula VII.). These latter values, however, in accordance with chemical usage, may be called "little refraction-equivalents," since invariably $\mathfrak{N} > m$.

The values of \mathfrak{N} relate to a unit such that $\mathfrak{N}(\text{H}) = 0.004$. In order to facilitate a comparison of the numerical values, I introduced another unit which is so chosen that $[\mathfrak{N}](\text{H}) = 1.0$, and I call the value of $[\mathfrak{N}]$ the reduced refraction-equivalent. In these last-mentioned values, which are simply relative, we get rid of every assumption concerning the unit of density used. The other values of \mathfrak{N} and \mathfrak{N} are calculated on the base of the density of the air being unity. To convert these into numbers corresponding to the density of water considered as unity, we have merely to multiply by 773.45.

From what has been above said, it is very clear that I have attached proper importance to Biot's formula (VI.). But yet I am still compelled to profess my adherence to Newton's refractive power.

The reasons for this are in part theoretical and in part practical. I shall mention several in the sequel.

* *Mém. de l'Inst. Paris*, 1806, vol. i. p. 322.

The conceptions and propositions of the undulatory theory are fully in accord with the Newtonian refractive power. Thus Lorenz finds*

$$\mu^2 = 1 + Dx - D \frac{y}{1 + Dx},$$

a formula which also involves the idea of the refractive power.

Secondly, the previous formula (I.) agrees with the deductions in the chapter on dispersion. According to Cauchy, to whom we owe our present theory of dispersion,

$$v^2 = a_1 + a_2 k^2 + \dots$$

For empty space, in which we must assume the dispersion to be a minimum,

$$v^2 = a_1.$$

For every other medium in which there is dispersion the velocity diminishes; the velocity a_1 , for so-called empty space, is diminished. Accordingly

$$v_1^2 = a_1 - a_2 k^2.$$

By means of

$$\mu^2 = 1 + \text{const.} \left(\frac{1}{\lambda^2} \right),$$

we obtain a formula agreeing with the formula (I.) used by me.

Apart from these theoretical propositions, the relations demonstrable numerically appear to speak in favour of Newton's refractive power.

In consequence of the equations

$$\mu^2 - 1 = 2(\mu - 1) + (\mu - 1)^2$$

and

$$M = 2m + Dm^2,$$

$$m = \frac{1}{2}M - \frac{1}{4}DM^2 + \dots$$

The two formulæ (I.) and (V.) are inseparably connected, inasmuch as the values of Newton and Biot's refractive powers form mutually sums and differences which are influenced only in a secondary degree by the variable element, the density. Hence it is easy to perceive that the constancy of the one formula partly involves the constancy of the other. In all cases in which, as in gases, the density is small, m and $\frac{1}{2}M$ coincide. A similar agreement is shown in those cases where the variation of density is very trifling throughout the members of long series of bodies. For the latter reason, which must by no means be neglected, the homologous series in organic chemistry will not serve to determine between the above formulæ.

In order, however, to determine whether the one or the other

* Poggendorff's *Annalen*.

of these formulæ (I.) and (VI.) is better in accord with the facts, I sought to compare widely different modifications of the elements, since we may presume that these are subject to much less variations than complicated combinations. Calculation gives:—

		$\frac{\mu^2-1}{D} = M.$	$\frac{\mu-1}{D} = m.$
Carbon . .	{ Diamond . .	0·001820	0·000529
	{ Anthracite . .	0·001829	0·000664
Sulphur . .	{ Vapour . . .	0·000492	0·000246
	{ Solid	0·002017	0·000659
Phosphorus.	{ Vapour . . .	0·000626	0·000313
	{ Solid	0·002437	0·000784
Arsenic . .	{ Vapour . . .	0·000217	0·000108
	{ Solid	0·000650	0·000247

The comparison of these numbers leads us to maintain the Newtonian refractive power M , since the latter are either equal or simple multiples. In the Biot's formula m no such unconstrained agreement can be seen. Even the law of *simple* factors is here of no avail, although it is evident that undetermined factors are also necessary for m .

The law of simple factors, however, is a universally valid one for all physical properties of the elements; for the consideration of the atomic volumes, specific heats, &c. always leads us to the distinct proposition, "The functional values of analogous elements are either equal or simple multiples of one another."

Apart from the above examples, the values of the Newtonian refractive powers $[(\mu^2-1)V]$ for the elements show in the clearest manner the law of the homologous series.

In the following Table these values are given side by side with their factors:—

$\frac{\mu^2-1}{D} = M.$		$\frac{\mu^2-1}{D} = M.$
O . . = 0·000489 . (1)		As _g . . = 0·000216 . (1)
S _y . . = 0·000492 . (1)		N . . = 0·000596 . (3)
Sc . . = 0·001589 . (3)		P _g . . = 0·000602 . (3)
S _f . . = 0·002016 . (4)		As _f . . = 0·000641 . (3)
Al . . = 0·000846 . (1)		Sn . . = 0·000652 . (1)
Zr . . = 0·000854 . (1)		Si . . = 0·001247 . (2)
Bc . . = 0·001640 . (2)		Ti . . = 0·002520 . (4)
Tl . . = 0·000324 . (2)		Ba . . = 0·000332 . (2)
Ka . . = 0·000483 . (3)		Pb . . = 0·000479 . (3)
Na . . = 0·000670 . (4)		Ca . . = 0·000664 . (4)
Li . . = 0·001646 . (10)		Mg . . = 0·001290 . (8)

In contradistinction to this series of M , the Biot's refractive powers $\left(\frac{\mu-1}{D}\right)$ show, on the other hand, no such regular sequence of values. I shall here disregard the numbers which I have reckoned for m , and give below the numbers $[D(H^2O)=1]$ given by Haagen* :—

	$m = \frac{\mu-1}{D}$		$m = \frac{\mu-1}{D}$
Oxygen	0·1813	Phosphorus	0·4710
Sulphur	0·4606	Arsenic	0·2510

No agreement can be perceived from these numbers between the optical values of these substances (which are so nearly allied chemically), although such agreement was clearly manifested in the above comparison of the values of M .

There are some who certainly will not fail to point to the small deviations of M from the absolute values of the multiples. But I think that every one who considers the matter impartially will recognize the law of multiples as that which determines, in the first approximation, the numerical values of the elements. And hence I consider that the above comparison has sufficiently proved that the optical function of the elements shown by the Newtonian refractive power M follows the law of serial factors.

I consider that such a regularity, when we fully consider the analogous chemical series, furnishes a further proof of the correctness of the views which I have hitherto held.

Finally, I must mention the important fact that I have succeeded, by means of the optical atomic numbers (also a consequence of my equation I.), in deriving the crystalline form of compounds from the volumetric values of the elementary atoms occurring in the compound†.

Thus one relation follows another in an unconstrained manner, and no one is in contradiction with the facts. The hypotheses which have been hitherto forced simplify themselves in many respects, and unexpected new relations appear at every step.

In the preceding I have declared my adhesion to the hypothesis of simple factors, and I even consider such an hypothesis, similarly to the method of volumetric calculation, necessary to establish the truth and possibility of an optical function.

I must here add that the defenders of the formula $(\mu-1)V$

* Pogg. *Ann.* vol. cxxxi. p. 127.

† Schrauf, *Physikalische Studien*, pp. 240-248. Pogg. *Ann.* vol cxxx. p. 433. *Physik der Krystalle*, chap. xi. pp. 160-172.

cannot avoid a similar assumption. To prove this I will adduce the following examples.

From the latest treatise by Descloizeaux* (kindly sent to me) I select the observations for octahedral arsenious acid, As^2O^3 . In this,

$$\mu_p = 1.748, \quad d = 3.698, \quad P = 198, \quad D(\text{H}^2\text{O}) = 1,$$

$$\frac{\mu_p - 1}{D} = 0.2022, \quad P \frac{\mu_p - 1}{D} = m = 40.03.$$

Now according to Haagen (*loc. cit.*), referred to the density of water $m_p(\text{As}) = 20.02$ and $m_p(\text{O}) = 3.00$. Hence, by calculation from the elements†, we get for arsenious acid,

$$2m(\text{As}) + 3m(\text{O}) = m'(\text{As}^2\text{O}^3) = 49.44,$$

a number which differs from the observed one to the amount of 20 per cent.

If, going further, we reckon the indices of refraction from the elements, it follows from $P = 198$, $d = 3.698$, $m' = 49.4$, that the coefficient of refraction $\mu'(\text{As}^2\text{O}^3) = 1.916$, while direct observation gives $\mu = 1.748$.

The hypothesis of factors, or, as I call it, condensation, is therefore not superfluous.

Another example, which is at the same time of importance for the derivation of the elements, may be found in the terchloride of phosphorus, PCl^3 .

I have (1865) given $D(\text{H}^2\text{O}) = 773$, $m(\text{PCl}^3) = 0.0576$, $m(\text{P}) = 0.0243$, $m(\text{Cl}) = 0.0110$; these numbers being derived from direct observation. If now we reckon the value of the compound from those of the elements, we find at once

$$m(\text{P}) + 3m(\text{Cl}) = m'(\text{PCl}^3) = 0.0573.$$

The difference between observation and calculation scarcely amounts to $\frac{1}{2}$ per cent. If we put $D(\text{H}^2\text{O}) = 1$, my data change to $m(\text{P}) = 18.81$, $m(\text{Cl}) = 8.58$, and for chloride of phosphorus we should get from observation $m(\text{PCl}^3) = 44.52$, and from calculation $m' = 44.55$.

It is seen that no better agreement can be imagined between direct observation and calculation by means of the elements. And yet this very body (PCl^3) induced M. Haagen (*loc. cit.*) to derive for phosphorus in combinations the value $m'(\text{P}) = 14.6$, a number which departs considerably from that obtained by the direct observation of free phosphorus, $m = 18.8$ [if $D(\text{H}^2\text{O}) = 1$]. The reason of this deviation is to be sought for in the fact that

* "Nouvelles recherches sur les propriétés optiques des Cristaux," *Mémoires de l'Académie*, vol. xviii. Paris, 1867.

† The functions derived from direct observation are denoted in the sequel by m , those obtained by calculation by m' .

M. Haagen takes for chlorine a value 1.2 higher, $m'(\text{Cl}) = 9.79$, than corresponds to the observation of the isolated element.

Haagen, it is true, by means of this higher value for chlorine, gains the power of deriving the refractive equivalents of the chlorhydrocarbons under the assumption of a small mean error. But the above example of terchloride of phosphorus shows that the employment of mean values may easily lead to mistakes. I myself in my "Studies," in considering the organic series, could have adopted many more simple derivatives for \mathfrak{M} , if I had neglected the consequent great differences in the equivalent of dispersion \mathfrak{N} . If the dispersion be neglected, the mean differences for the refraction between observation and calculation from the elements are certainly small, and in many cases a very simple derivation from the elements is possible. But such a procedure is still one-sided.

Indeed the numbers for the compounds of chlorine give, as it appears, a very easy proof of the necessity of factors, namely:—

The difference between the observed values for chlorine and hydrogen (here, and afterwards, $D(\text{H}^2\text{O}) = 773$) is

$$m(\text{Cl}) - m(\text{H}) = 0.011 - 0.002 = 0.009;$$

and this difference ($\text{Cl} - \text{H}$) appears on deriving many of the chlorhydrocarbons.

If we compare such substitution-products (compare my "Studies"),

- (a) Chloride of ethylene, $\text{C}^2\text{H}^4\text{Cl}^2$, $m = 0.0410$ $\Delta = 0.0147$.
 Bichloride of ethylene, $\text{C}^2\text{H}^3\text{Cl}^3$, 0.0557
 (b) Chlorobenzol . . . $\text{C}^6\text{H}^5\text{Cl}$, $m = 0.0646$
 Trichlorobenzol . . . $\text{C}^6\text{H}^3\text{Cl}^3$, $0.0869 \Delta = 2(0.0112)$,

the differences Δ derived from the observations for ($\text{Cl} - \text{H}$) differ considerably from the former number, 0.09. In the first case $\nabla = 0.0057$, in the second $\nabla' = 0.0021$. If in this calculation $D(\text{H}^2\text{O}) = 1$, then $\nabla = 4.40$, a deviation which attains almost 10 per cent. of the value of the observation itself.

Similar facts and the necessary consideration of the dispersion have caused me in my "Studies" to retain the theory of simple factors, and to fit this as closely as possible to the molecular processes of chemical substitution. If in this investigation it has appeared that all the atoms of an element which occur in a compound are not of equal value, and if, further, the theory of Newton's equivalents of refraction leads to the assumption of several modifications of the elements, both of these are in accordance with modern chemistry; for chemistry already recognizes four modifications of carbon, distinguished by their atomicity.

XXV. *On the Experiment of Mahomet's Coffin.*

By G. JOHNSTONE STONEY, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

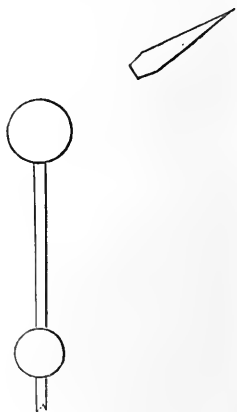
Dublin, August 12, 1868.

MR. W. F. BARRETT, during a visit to Dublin last spring, showed an uncommonly beautiful electrical experiment, which I understood him to say was described in the older treatises on electricity, but appeared to have since dropped out of recollection. I do not find any reference to it in such books as I have access to. You may perhaps, then, not deem it a waste of your space to recall attention to it, and insert an explanation of a phenomenon which at first sight seems very marvellous.

The experiment is easily made. Raise the knob of a Leyden jar to a considerably greater height than is usual. This may be done by boring a hole in the top of the ordinary knob of the jar and inserting into it a stem 3 or 4 decimetres long, carrying another knob. Charge the jar, and then drop down a little strip of gold-leaf towards the knob from a height of a couple of decimetres. The strip may be from 3 to 5 centims. long, and an attempt should be made to cut it into a form somewhat sharper at one end than the other. This is best done by heating a sharp penknife so as thoroughly to dry it, and then cutting the gold-leaf with tolerable pressure, and by a motion resembling minute strokes of a saw. All that need be aimed at is that the fragment have either more or sharper jags at one end than the other. If, when it is dropped towards the knob, the sharper end happens to be directed downward, the strip of gold will touch the knob and immediately dart away again; but if it be dropped with the blunter end downwards, it will approach without touching the knob and take up a position in its vicinity apparently without any support. There it will continue in mid air so long as the jar remains sufficiently charged.

The explanation may be conveniently divided into two parts—one respecting the distance at which the fragment of gold-leaf will establish itself, and the other explaining how its weight is supported.

At all distances the strip is charged by induction, electricity unlike that of the jar (which we shall suppose positively charged) being driven to the further and sharper end, while like electricity



is drawn to the nearer end, precisely equal quantities of both kinds being simultaneously evolved. Now, if the strip be far off, in which case the positive electricity of the knob will act with very nearly equal effect upon the two ends of the strip, positive electricity will escape from the sharp end more freely than negative from the blunter end. The strip will therefore become negatively charged and be attracted. On the other hand, if the strip be very near the knob, the difference between the forces with which the electricity of the knob acts upon the nearer and further ends will become so great that the negative electricity will be made to flow inwards, in spite of the bluntness of the inner end, more copiously than positive electricity escapes outwards. Here, then, the strip would become positively charged and be repelled. There is therefore an intermediate distance at which neither the repulsion nor the attraction preponderates; and at this distance the strip in the first instance places itself, or at least would place itself if it were devoid of weight.

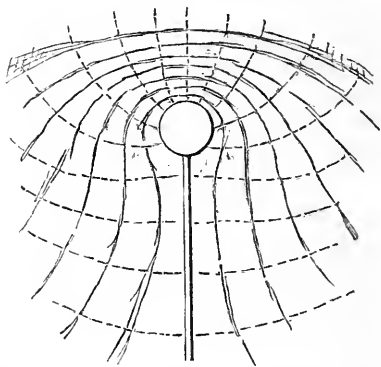
The gold-leaf may be regarded as acted on by three forces:—the relief of atmospheric pressure due to the attraction of the electricity of the knob upon the negative electricity of its nearer end (this is a force directed towards the knob); the similar relief of atmospheric pressure due to the repulsion of its positive electricity, which is a force acting outwards from the knob; and the weight of the gold acting downwards. If the two former forces lay in a right line, the weight of the gold-leaf could not be supported. We must therefore inquire into the disposition of the lines of force in the neighbourhood of the knob of a charged jar.

This may be done by first considering the form of the equipotential surfaces*, to which, as is known, the lines of force are

* If an electrified body were brought near the knob of a charged jar, it would by induction alter the distribution of electricity in the jar. But we may conceive a Leyden jar to be first charged, and that then the coatings, stem, and knob become perfect non-conductors. In this hypothetical case the approach of other electrified bodies would not alter the charge in the jar. Let us suppose that the charge in the jar is positive, and that a particle charged with one unit of positive electricity is taken from a position in which it is not acted on by any electrical forces, and brought up to a point P in the neighbourhood of the knob. The repulsion of the charge in the jar of course opposes this motion; and it will therefore require the expenditure of a certain amount of mechanical energy, or, as I should prefer to call it, *labour*, to overcome the electrical repulsion. This labour may be expressed in foot-pounds, kilogrammetres, or other more convenient units. The number which expresses the amount of labour that we must apply in order to carry the particle up to the point P is what is called the *potential* at that point of the electricity in the jar.

If all the points be found which require the same expenditure of labour as P does to carry the unit particle of electricity from a neutral position up to them, they will all lie on a surface which is called the *equipotential surface* passing through P.

everywhere perpendicular. The charge on the ball of a Leyden jar is evidently most intense on the top of it, and gradually feebler down its sides. Hence the force exerted on a molecule of free electricity close to the knob is greater if the molecule of electricity be above the ball than if it be at the same distance beside it. And accordingly, if the equipotential surfaces corresponding to the potentials V , $V-\alpha$, $V-2\alpha$, &c. be drawn, V being the potential of the inside coating of the Leyden jar, and α being a small quantity, the first of these surfaces will be the surface of the ball and stem, and the others of the form represented by the unbroken lines in the diagram, since it is a property of these surfaces that



the interval between them at any point of space is inversely as the intensity of the force at that point, so that they lie closest to one another above the ball, where, as we have seen, electrical attraction and repulsion are most intense, and further asunder at the sides, where the electrical force is feebler. Having now ascertained the general form of the equipotential surfaces, we have only to draw perpendiculars to them everywhere in order to have the directions of the lines of force. It thus appears that these latter are curved upwards, as represented by the dotted lines in the figure, and that the repulsive force acting on the further end of the gold-leaf is accordingly more steeply inclined to the horizontal than the attractive force acting on its nearer end. Hence they have a resultant acting upwards, which may balance the weight of the gold-leaf.

If this resultant be too strong, it will draw the gold-leaf up into a higher position, in which the lines are less curved. If, on the other hand, the weight preponderate, it brings the gold-leaf into a lower position, in which the electrical forces acting on it become more inclined to one another. Now the effect of the increased inclination of these forces in the lower position, which tends to increase their upward component, may or may not preponderate over the reverse tendency of their diminished strength. This will depend on the size of the knob, and on its elevation above the outer coating of the Leyden jar; for the smaller the knob, the more intense will the accumulation of electricity on its top be as compared with that on its sides, and, accordingly, the more will its lines of force be bent upwards. And, again, since

all the lines of force must ultimately find their way to the outer coating or some other neutral position, those which begin by curving upwards must after a time incline in the opposite direction; and this will take place nearer to the knob the shorter the stem is which raises it. But if the knob be not exorbitantly large, and if it be sufficiently stilted up above the jar, the vertical component of the electrical forces acting on the strip will go on increasing from the top of the ball and for some distance down its sides; so that if the gold-leaf be not too heavy, it will find a position of stable equilibrium.

Again, if the resultant of the electrical forces be not directed through the centre of gravity of the gold-leaf, a couple will be formed which will incline the strip of gold-leaf so that, instead of lying along one of the lines of force, it will lie in some degree athwart them. But the new directions of the electrical forces will manifestly produce an opposing couple which will bring about equilibrium.

And, again, if the electrical forces produce a horizontal as well as a vertical component, this will cause the slip of gold-leaf somewhat to approach or withdraw from the knob. In either case its position may, and probably will, be different from that in which it discharges positive and negative electricity from its two ends in equal quantities, and in which alone its own electrical condition can remain constant. If, then, when the strip is in this position of constant charge, it happen that all the forces acting on it produce *e. g.* an *inward* resultant, it must approach the ball and thus come into a position in which it quickly becomes positively charged, from which it is therefore again driven outwards beyond the position of constant charge into a situation in which it becomes negative, from which it is therefore drawn in again. It thus flutters between two positions on either side of the position of constant charge. I have not seen this motion in the main body of the strip, although it certainly exists; but I have seen a fragment which happened to be slenderly attached to the side of a strip quivering with a motion which was no doubt due to it.

It now only remains to notice some minor details. I have seen the strip spin on its longer axis. This was doubtless because the jags, which it is difficult to avoid in cutting gold-leaf, happened on that occasion to turn upwards along one side of the strip and downwards along the other, so that it was moved like the electrical fly-wheel. Similarly, if the jags along one side allow a freer escape of electricity than those along the other, as often happens, the little strip traverses horizontally in a cone round the vertical line through the knob; for the little strip of gold commonly assumes a position in which its transverse dia-

meter is horizontal. If one side of the strip were lower than the other, it would be in a situation in which the electrical forces are both feebler and more horizontal. The attractive forces therefore tend to restore the transverse diameter to its horizontal position, the repulsive forces tend to draw it from it; and which of them it will obey depends on whether the attractive or repulsive forces have the greater moment round the longer axis of the strip. But if the moment of the repulsive forces preponderate, the plane of the strip becomes vertical, and any inequality of the jags along its sides will carry it up or down from its position of equilibrium, so that it probably soon flies off. It appears, therefore, to be only in those cases in which the moment of the attractive forces preponderates that the experiment is likely to succeed. Hence it would seem that a good shape for the strip would be that of an arrow-head with a sharp point and two blunt barbs. Finally, I have seen the strip take up a position several centimetres below the knob, showing that even lines of force which started from the stem curved upwards.

I am, Gentlemen,

Your obedient Servant,

G. JOHNSTONE STONEY.

XXVI. *A New Theory of Vision*.

By SAMUEL ROWLEY, M.A.*

MY purpose in the following communication is to offer a theory of vision which I believe has not been heretofore advanced, and which I venture also to believe to be the true one. But before entering upon its consideration it may be proper briefly to state, with reasons for rejecting them, the theories hitherto advanced.

The theory of Aguilonius supposes that all the images impressed on the retinae at any given instant are seen in a plane perpendicular to the plane of the optic axes at their point of convergence, and parallel to the line joining the centres of the two pupils—and consequently that the two images of any object situated in this plane, occupying, as they necessarily do, one and the same place, are seen single with two eyes.

If any person, whom a little practice has fitted to make such experiments, will place at some short distance (say, 3 inches), in a line parallel to the eyes two small objects, one directly before the central point of the interval between the eyes, and

* [We are indebted to the author for proof-sheets of this paper in advance of the issue of the September Number of Silliman's Journal, to which it was first communicated. Eds.]

the other at a distance from this, say, $\frac{3}{4}$ of an inch, and direct his optic axis to the former, he will not see the two images of the latter as one, though their object is situated in the plane mentioned, but as two distinct images, the one beyond the other.

Experiments to the same effect might easily be multiplied; but the one given abundantly demonstrates the inaccuracy of the theory of Aguilonius.

The theory of *corresponding points*, so called (as held on physiological grounds by Galen, Alhazen, and Newton), cannot be denominated a theory of vision. It is directed to the accounting for but one of the phenomena of vision, that of single sight with two eyes. It does not deal with visible direction and distance, whereas these conditions, governing, as they do, external visible position, include the result of single sight with two eyes as a necessary consequence.

The theory of vision propounded by Dr. Smith in his 'Optics,' and that by Dr. Wells, from which he derives his explanation of the phenomenon of single vision with two eyes in his 'Essay on Single Vision with Two Eyes,' are practically (as I trust will satisfactorily appear hereafter) merely attempts to formulate the misapprehensions by consciousness of true visible distance which take place under the influence of habit. A simple experiment subversive of both these theories may be made thus. Let two small objects, as pins in a board, be placed directly in front of the middle point between the eyes, the one at some short distance, say, 6 inches, and the other at some more considerable distance, say, 18 inches, and let other two small objects be placed on opposite sides of the near one in lines between the eyes and the far object, and the optic axes be directed to the near object; then the two images of the remote object will be found at no greater distance from each other than the images of the objects at the sides of the near one, or all will be apprehended as lying in the figure of a rectangle.

But by the theory of Dr. Wells* the two images of the remote object (since this object lies in the direction which he has named the common axis) are seen in the optic axes, the right eye's image in the left eye's axis, and *vice versó*, and at a distance from each other (if located at the same distance from the eyes as the object)† of $4\frac{8}{10}$ inches, whereas the images of the two lateral near objects, since these occupy the line named *horopter*, will be seen, in the places of the objects, at a distance from each other of $1\frac{6}{10}$ inch‡.

As to the two images of any object more or less distant than

* Essay upon Single Vision with Two Eyes, p. 38. London, 1818.

† Ibid. page 27. ‡ Ibid. pages 5 and 46.

the convergence of the axes, the theory of Smith places them in lines drawn from the eyes to the object somewhere between the place of the object and the point of convergence, "but not very far from the real place," *i. e.* the place of the object*. Hence, while the two images of the object on either side of the point of convergence will be seen coincident with their object at a distance of $1\frac{6}{10}$ inch from the two images (seen in like manner) of the object on the other side, the two images of the remote object will be seen at a much less distance from each other, or, following the proportions of the construction given us by that author, at a distance of $\frac{5}{10}$ or $\frac{6}{10}$ inch.

Aguilonius is the only author who, meeting the question in its full extent, undertakes to assign for each image both the line of visible direction and the absolute visible distance from the retina on the line of visible direction.

Sir David Brewster indeed gives both conditions of external visible position for the images falling *on the vertices of the retina*. But for every other image he merely assigns Kepler's line of visible direction, being silent regarding the visible distance—the essential complement of the former in determining visible position.

I will now propose and proceed to put to the test of experiment what I believe to be the true theory of vision—premising the remark that in establishing its existence it will not render the proof offered less exact if I do not undertake to determine the precise point where a given ray† strikes the retina, nor the precise point at which the line of visible direction crosses the optic axis, provided we admit (what I think will not be disputed) that where the angles with the optic axes of two rays falling one on each cornea are equal, the distances of the images on the retina from the extremities of the axes are equal, and provided we admit (what I also think will not be disputed) that where the angle with the axis of one of the rays is but *very slightly* increased, the angle formed after decussation is not appreciably *unequally* increased—and further premising that I will neglect any effect of refraction, and also suppose a coincidence of the line of the ray with the line of visible direction.

All the component points of the total impression on the retina of either eye, before becoming cognizable mental phenomena, are simultaneously, and in lines drawn from them through a point situated, say, a little behind the centre of the crystalline lens, referred outward to a surface parallel to the impressed surface of the retina conceived to be simply expanded coextensively with the impression

* Smith's 'Complete System of Optics,' art. 137.

† Here, and wherever in this paper the word *ray* is used, it is intended to signify the central or axial ray of the pencil.

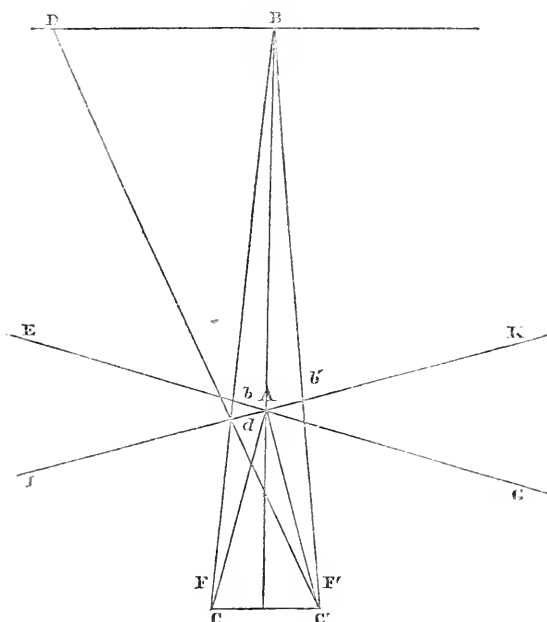
us expanded by outward reference, and cutting at right angles the optic axis of that eye at the point where it intersects the optic axis of the other eye.

Or, to change the form of statement, the entire impressions on the retina, before becoming objects of consciousness, are projected in space upon surfaces bisecting each other (perpendicularly to the plane of the axes) at an angle greater or less according to the distance—the component points of each impression being simultaneously referred outward in lines passing from them through a point a little behind the centre of the crystalline lens, but, excepting the expansion and the inversion resulting from the crossing in the eye of the directions of outward reference, undergoing no change of relative position—the distance between the planes passing at right angles to the optic axis through any two of the successive concentric zones of points which make up the retinal impression continuing the same.

I do not mean here to assert that an outward projection actually takes place, but that the effect of such a projection produced by an intermediate process of mind is presented to the consciousness.

On a suitable plane surface take two points, C, C' (fig. 1),

Fig. 1.



$2\frac{4}{10}$ inches apart, and in a line bisecting at right angles one

joining these points take other two points, A, B, distant respectively from the point of bisection 6 and 18 inches. Draw the straight lines CA, C'A, and perpendicular to them at A the two lines EG, IK. Through the points F, F', taken in the lines CA, C'A, $\frac{1}{10}$ inch from C, C' draw the lines FB, F'B, intersecting EG, IK, at b, b' . Through the points F' and d (a point in IK where a line passing through b perpendicular to CC' intersects it) draw a line. In this line, where a line passing through B parallel to CC' intersects it, take the point D. The distance of this point from B will be $4\frac{2}{10}$ inches.

Now suppose the points C, C' to represent the centres of the eyes; points F, F' the focal centres; A, B, D the situations of luminous points; the lines CA, C'A the optic axes produced; the lines EG, IK lines of intersection where two surfaces slightly convex toward the eyes, perpendicular respectively to the optic axes at their point of meeting, cut the plane of the axes; BF, BF' lines of luminous rays passing from the point B through the points F, F'; DF' the line of a ray passing from D through F' to the retina; b the point where the line BF intersects the line EG; and d the point of intersection of DF' with IK. Then, if the above-enunciated proposition be true, to wit, that an impression made by a luminous point on the retina of the right or left eye becomes visible and is seen where a line drawn from it through a point a little behind the centre of the crystalline lens pierces a slightly convex surface perpendicular to the optic axis of that eye at the place of its meeting with the axis of the other eye, the image of the point at B will be seen from the left eye at b , and the image of the point at D from the right eye at d , both in the same perpendicular of no variation*, or the image of the point at B will be seen by the left eye a little behind the place in which the image of the point at D will be seen by the right eye.

The experiment may be made thus:—Take a thin rule (as a plane scale) 12 inches in length with the right hand, and another rule with the left hand. Hold the rule in the right hand with its wide sides perpendicular to the plane of the horizon, in a line running away from the middle point between the eyes so that the near end shall be distant 6 inches, and the remote end rest upon a horizontal bar of a well-lighted window at right angles to the rule, and parallel to the eyes.

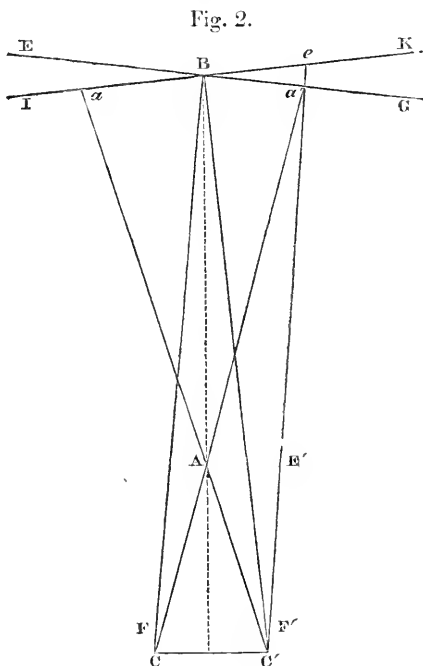
To obtain the former distance with accuracy, it is necessary to have two small objects, as two pins, placed equidistantly on opposite sides of the point of intersection of the bar by the rule, distant from each other $4\frac{8}{10}$ inches; and when these shall be

* By *perpendicular of no variation* is meant a line perpendicular to the base of vision, for the definition of which see note, p. 203.

seen across (or lie in the axes exactly fixed upon) the near end of the rule, that distance for eyes $2\frac{1}{6}$ inches apart will be accurately found. Now fix the axes of the eyes on the near end. The images of the remote end will appear as two at the sides, that of the left eye at some unknown distance on the left, and that of the right eye in like manner on the right. Holding the rule in the left hand in the same position with respect to the plane of the horizon as the other, apply its remote end on the window-bar to the left of the remote end of the rule in the right hand, so that the right eye's image of the former will cover the left eye's image of the latter. Now measure the distance between the far ends on the bar. If all the conditions of the experiment shall have been exactly complied with, the distance found will be $4\frac{9}{16}$ inches, as predetermined in the diagram.

The experiment, then, shows that the geometrical consequence of the principles of the proposition, as illustrated in the diagram, holds good in actual practice, and that when the axes are fixed upon the images of an object placed as a luminous point at A, the left eye's visible image of an object placed as a point at B in the line F B, is seen a little beyond where the right eye's visible image of an object placed as a point at D in the line drawn through F and *d* is seen, both in the same line of no variation. It is obvious by the proposition that it ought not to affect the "conjunction" of the images, wheresoever in the line passing through F' and *d* the object might be placed and this will be found to be the fact. It was placed opposite B at D merely for convenience of applying the diagram in actual experiment, and to procure a correct appreciation of the interval between the images.

Suppose, in the second place, the axes represented by C B,



C' B, fig. 2. Then by my proposition the left eye's visible image of the luminous point at A should be seen at a' in a surface cutting the axis F B at right angles in B (and the plane of the axes in the line E G), and the right eye's image at a .

But if the left eye's image of the point at A is seen at a' , then by the same proposition the right eye's image of the point at E' opposite A in a line between F' and e , a position in I K directly beyond a' should be seen in the same line of no variation a little beyond a' at e . If the experiment be made, it will be found that the right eye's visible image of an object placed as a point at E', nearly $1\frac{6}{10}$ inch from A, will be seen a little behind the place where the left eye's image of an object placed as a point at A is seen.

So the two middle images of the pins placed as a test of distance in the first of these experiments are seen united at the intersection of the surfaces of vision.

If the surfaces of vision were planes, since their line of intersection would then coincide with the line of intersection of the vertical planes of the axes, it is evident that the images of these pins longitudinally bisected by the latter planes would exactly coincide. But, owing to the slight curvature of the surfaces of vision, the line of intersection of these surfaces immediately begins very gradually to recede from that of the vertical planes of the axes, and the longitudinal axes of the images of the pins to cross over and fall on opposite sides, thereby preventing a mathematical coincidence of the images.

This effect of the departure of the intersection of the surfaces of vision (supposing the optic axes parallel to the horizon, to which the pins are perpendicular, to be directed to the bottoms of the images) may be counteracted and a mathematical coincidence of the images nearly produced by inclining the pin whose image is seen by the left eye a very little to the left, and the other duly to the right. Should this small inclination be given with such a curve that a line drawn from any point of the axis of the pin to the focal centre of the eye would intersect the line of intersection of the surfaces of vision, then an exact mathematical coincidence of the images would be secured.

The approximate result may be obtained if two rulers*, or the legs of a pair of compasses†, be so placed that their longitudinal axes shall lie in the vertical planes of the optical axes.

It is evident that for the two objects in the vertical planes of the axes, we may put a single object at the intersection of these planes‡.

But to make the accurate performance of these experiments

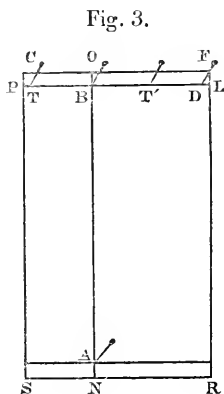
* See Wells, p. 36.

† Smith's 'Optics,' art. 977.

‡ Wells, p. 49.

easy, some special instrument must be provided. A piece of pine board whose opposite great surfaces are rectangles parallel to one another, 13 inches long and 8 inches wide, with five pins, furnishes the means for obtaining accurate results where the distance between the near and far points does not exceed 12 inches*.

To use this contrivance (supposing the distance from the middle of the line between the centres of the eyes to the near objective point to be 6 inches, and from the near to the far objective point 12 inches), in the surface $S R F C$ (fig. 3), $\frac{1}{2}$ an inch from the short side $S R$, and 5 inches from the long side $R F$, take the point A . Through this point and at right angles to the side $S R$ draw a line $N O$. In this line, 12 inches from the point A , take the point B . Through this point, and at right angles to this line, draw another line $P L$. In this latter line take the two points T, T' , each distant $2\frac{1}{10}$ inches from the point B , and $4\frac{8}{10}$ inches from the other; and finally $4\frac{9}{10}$ inches from the point B take the point D . At each of the points so taken set a pin. Now place the board in a horizontal position a little below and parallel to the plane of the axes, with the pin at A 6 inches from the middle of the line joining the centres of the eyes, and, together with the pin at B , in the vertical plane which bisects this line at right angles; which disposition of the pins is had when, looking at the images of the pin at A , the left eye's image of the pin at T and the right eye's image of that at T' are found lying in the axes.



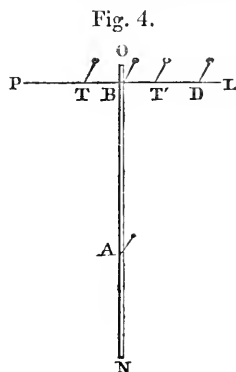
When now the axes are fixed upon the images of the pin at A , the right eye's image of the pin at B will be seen in the same line of no variation as the left eye's of the one at D , and at a point a little beyond where the latter is seen; and when they are fixed upon the images of the pin at B , the left eye's image of that at A will be seen in a position a little before where the right eye's image of a pin, set nearly $1\frac{6}{10}$ inch directly to the right of A , will be seen, the distance between these being sensibly less than that between the images received from B and D when the axes were converged toward A .

* Another board of the same width and 18 inches long, serving as a support to this, to be held with one end against the nose by the hands pressing both pieces at the sides, will be found serviceable for maintaining steadiness of position.

A still better instrument is a small wooden cross consisting of a slender bar and sliding transverse piece, which may be any slip of wood (as a piece of common lath) notched at the centre so as to receive the longitudinal bar in its full thickness at right angles, and also capable of being shifted and fixed by wedging at any point.

For all experiments in which the distance of the remoter of the two stations designated A and B, figs. 1 and 2, is not greater than 18 inches, the length of the bar should be 18 inches, and that of the transverse piece 12 inches.

To apply this instrument, instead of the window-bar and rulers in the first of the examples taken, fix the transverse piece at the distance of 17 inches from one end of the shaft, and laying the cross on a table with its level side upward, draw on the upper side of the shaft lengthwise the bisecting line NO , and lengthwise on the upper side of the transverse piece the bisecting line PL . At B , the point of intersection of these lines, set a pin, and at the point A , 12 inches from the point B , in the line NO , also one. At the points T, T' in the line PL , each distant $2\frac{1}{10}$ inches from B , set two other pins, and at D , $4\frac{9}{10}$ inches from B , another. Then placing the cross



so as to lie with the extremity N of the shaft against the upper part of the nose, and its upper surface a little below and parallel to the plane of the axes, and keeping the head fixed, look with each eye in rapid succession at its image of the pin at A . If the right eye's image of the pin at T , and the left eye's of that at T' , be seen in these directions of the axes, the pins at A and B are 6 and 18 inches distant from the middle point of the line joining the centres of the eyes. But if they should fall within or without the axes thus directed, then the transverse piece, together with the pins at A and B , 12 inches apart, must be shifted slightly forward or backward until the requisite position is found.

Holding this instrument in the same position with respect to the eyes as that just above given to it for ascertaining distance, turn it toward a strongly illuminated white wall or sheet of white paper, or (which is better) the clear sky. If now the axes be fixed upon the images of the pin at A , the right eye's image of the pin at B and the left eye's of that at D will be seen lying in the same line of no variation, the former a little beyond the latter. But if, having transferred the transverse piece to the

distance of A, the axes be fixed upon the images of the pin at B, then the image of the pin at A seen by the left eye, and that of a pin placed nearly $1\frac{6}{10}$ inch directly to the right of A seen by the right eye, will be found lying in the same perpendicular of no variation, the latter beyond the former, the distance between the two images being less than in the first case.

In experimenting with this instrument, the position of the image of the pin at B seen by the left eye in a surface at right angles to the optic axis of that eye is visibly realized. For when the axes are fixed toward A (as in the first instance), the image of the transverse piece seen from the left eye visibly has a position at right angles to the axis of that eye, and intersects the corresponding image of the right eye, in a line of common intersection crossing each image where that image, whether referred back to the retina or to the distance of the transverse piece itself, would be pierced by the optic axis.

In making these experiments, care should be taken not to turn the axes into the oblique directions, to do which there is always a strong inclination; for then the images which were seen in those directions as described, the one before or behind the other, would be seen coincident.

I will now proceed to answer an apparent objection. In the first demonstration under my proposition, I undertake to show that the impression on the left eye from B becomes visible and is seen at *b*, by showing that it is seen a little behind where the impression on the right eye from an object placed at D is seen by the right eye, arguing that nothing else than a projection in the manner described can account for the local relationship in which these impressions are seen. The evidence of consciousness, then, that these impressions are seen in a certain local relationship, is made the groundwork of my argument. Now it may be urged that the evidence of consciousness is equally strong that the impression from the object at B on the left eye, when seen, lies in the axis of the right eye at the actual distance of the object itself at B*.

In the first place, let us determine whether consciousness, when carefully examined, does really continue to find this impression elsewhere than at *b*. To reveal in consciousness the true distance in a lateral direction of the visible image at *b* of an object at B (fig. 1), employing the pins and board, place a pin at *b*. The left eye's image of the pin at B will not be recognized to the left of the left eye's image of it.

Again, take three equal small rings of pasteboard. Attach to each, in the direction of a radius produced, a pin, by cementing with a drop of sealing-wax the head of the pin to the edge of the ring.

* See Wells, pp. 38 & 27.

At B, fig. 3 (the board duly widened), at a point $4\frac{8}{10}$ inches to the right of B, and a point of $4\frac{8}{10}$ inches to the left, station these pins, so that the rings shall be parallel to the eyes of the observer. At the points A, T, T', set simple pins. Now on converging the axes toward A, till the right eye's image of the pin at T and the left eye's of the pin at T' are hidden in the combined images of the pin at A, the images of the rings will be apprehended in their true visible places.

To make another experiment of the kind to which the last belongs, on a glass of a window situated at a convenient height stick a number of wafers, placing their centres in a horizontal line and separating the adjacent centres by a distance less than that between the centres of the eyes, but being extremely careful to make all these distances exactly equal. Direct the axes through the centres of two adjoining wafers, each axis through that centre lying on its own side, and presently all the visible images (except usually the left terminal image of the right eye's series and the right terminal one of the left eye's) will be recognized by consciousness in their true visible positions.

If the adjoining centres of the wafers are very close to each other (as a little over $\frac{1}{2}$ inch, supposing the wafers to be $\frac{1}{2}$ inch in diameter) and the eyes at a short distance, say, 4 inches, *the curvature of the surface of vision will be plainly appreciated*, and if the plane of the axes be turned on the line joining the intersection of the axes to the centre of the interval between the eyes from a coincidence to an angle with the horizontal plane passing through the centres of the wafers, *the curvature of each eye's series will be separately appreciated*.

An experiment of the same kind may also be made by means of the similar figures lying at equal distances in the surface of a papered wall.

If the experimenter will station himself facing such a wall and converge his optic axes so that the axis of his left eye shall pierce the centre of some figure in front of him, and that of his right eye the centre of the next one to the left of the other, then shortly each eye's image of the papered wall will, to his consciousness, take its true visible position at the distance of the convergence of the axes—the images of the figures appearing smaller or larger, according as the distance of the point of convergence is less or greater. A variation (from the standard) of the distance between the centres of the figures of the wall will produce a seeming elevation or depression of the visible images. The cause of this last, as well as that of the principal phenomenon, will be explained when it shall have been explained why the lateral images of an object which is at a greater or less distance

than that of the convergence of the axes are not apprehended in their true visible positions in the surfaces of vision.

Why, then, does consciousness mistake, precisely as we find it doing, the true place of the visible image of any object, lying not at the point of intersection of the axes, nor at any point equally distant with this from the base of vision*?

My answer is, that under tendencies resulting from experience, consciousness apprehends erroneously the distances really presented for its action according to the laws of vision, and with respect to each lateral image apprehends the distance of the surface of vision from the base of vision, and the distance in the surface from the image at the centre, for the distances that would exist between these surfaces and these images if the given image now laterally and obscurely seen were, with its corresponding image of the other eye, directly and distinctly examined at the centre.

Hence the place in which such an image seems to be seen.

For example, let the axes be turned to the images of an object at B (fig. 2). They will be seen at B, the left eye's $2\frac{4}{10}$ inches to the left from its image at a' of the object at A, and the right eye's $2\frac{4}{10}$ inches to the right from its image at a of the object at A, each in a surface of vision measuring, from the apex, 18 inches from the base of vision.

Let, now, the axes be turned to the images of the object at A (fig. 1). These will now be seen at A, that of the left eye's $\frac{8}{10}$ inch to the right from its image at b of the object at B, and the right eye's $\frac{8}{10}$ inch to the left from its image at b' of the object at B, each in a surface measuring, from the apex, 6 inches from the base of vision.

At first, then, the left eye's image of an object at B, when particularly examined, will always be apprehended at the distance of $2\frac{4}{10}$ inches to the left from the image at a' of an object at A in the surface of vision 18 inches from the base. Hence, after due experience, will arise a tendency of mind to apprehend this image at a distance of $2\frac{4}{10}$ inches to the left from the image of the object at A in the surface of vision 18 inches from the base. Therefore, when the axes are turned toward the images of the object at A, the mind will be influenced by this tendency, with respect to the left eye's image at b of the object at B, to apprehend 6 inches, the distance of the surface of vision from the base, as 18 inches, and $\frac{8}{10}$ inch, the distance in the surface from the image at A of the object at A, as $2\frac{4}{10}$ inches.

Again, the effect of experience in regarding directly the images

* By *base of vision* I mean a plane cutting perpendicularly, at the centre of the interval between the eyes, a line drawn through this point and the point of convergence of the optic axes.

of the object at A will be a habit whereby, when the axes are directed to the images of the object at B, the mind will be led to mistake $2\frac{4}{10}$ inches (the distance in the surface of the left eye's image at a' of the object at A to the right of its image at B of the object at B) for $\frac{8}{10}$ inch, and 18 inches (the distance of the surface of vision from the base) for 6 inches; and $2\frac{4}{10}$ inches (the distance of the surface of the right eye's image at a of the object at A to the left of its image at B of the object at B) for $\frac{8}{10}$ inch, and 18 inches (the distance of the surface of vision from the base) for 6 inches.

Thus, since the more remote parts of a solid object will seem to be seen at equally more remote distances in the image, and the entire image at a distance equal to that of the object, the image will seem to be seen with the same form and magnitude as really pertain to the object*.

We are now also prepared to explain the cases in which the visible image, though the object be situated at a greater or less distance from the base of vision than the point of convergence of the axes, is accepted by consciousness in its true place, as in the experiments with the equidistant pins, with the wafers upon glass, and the figures on the papered wall.

In these cases it will be perceived that any two images seen as one, though proceeding from two different objects, have the same positions in the surfaces of vision as they would if they proceeded from a single object placed at the intersection of the lines drawn from the two different objects to the eyes. Further, so great is the propensity to appreciate singly, that any slight discrepancy between these images and those produced by one and the same object will not prevent an acceptance of a correspondence; and since all the equivalent single objects would lie at the same distance, no habit can exist to influence the mind with respect to any images to locate the surfaces of vision at a greater or less distance from the base of vision than the true. Hence all the images will be apprehended in their true visible places. But if the interval between any two objects of such a series should differ from the other intervals, then for the two (of the four images presented) which are accepted as corresponding, or capable of being seen singly in direct vision, if these should be lateral (or if central, then for all the rest), from the necessary principle of habit above laid down, the surfaces of vision, measured from the common centre, will seem to exist at such a dis-

* In ordinary lateral vision (*i. e.* vision had at the sides of the line of intersection of the surfaces of vision) two corresponding images, or the corresponding parts of two complex images, at the concurrence of the axes are appreciated singly by reason of inattention to, or neglect of, one of them.

tance from the base of vision as that at which they would if these images were directly seen in the axes.

In this experiment with the papered wall, with every movement of the head a movement of all the images in the same direction takes place; because with the eyes fixed toward any two figures, a movement of the head in any direction will carry in the same direction all the points of intersection of the lines of direction, and consequently all the images in the two surfaces. So when the axes are made to pass through any two of a series of equal and equidistant objects to a point beyond, as in the case of the experiment with the wafers upon glass, a movement of the head in one direction will produce a movement of the images in a contrary direction.

There are other circumstances incident to a lateral image in the surface of vision, besides distance from its twin, which will produce habits operating to defeat a correct appreciation of the two distances.

Such a circumstance is the visible magnitude.

Thus, whatever the visible magnitude of an aggregate lateral image, which has been often seen in direct vision, this magnitude will be taken for that found on particular examination, and the visible distance for that corresponding to that magnitude. But since lateral images in the surface of vision not distinguishable from each other by consciousness may result from objects of very different magnitudes placed at suitably different distances, a given magnitude of image will not always excite the same tendency of mind.

To illustrate this, take the board with pins placed at A and B (fig. 3), depress the far end till the surface is hidden by the near end, and stopping the light flowing from the near pin to one of the eyes by interposing near the eye an adequate object, fix the axes of both toward the remote pin. As soon as the eye has ceased its alternations of adjustment (which have been performed so rapidly as to keep up a sensibly sustained distinctness of both images), and no longer experiences the sensation of focal adjustment to the near pin (which has excited a tendency of mind to mistake the image at the distance of the object), the now shadowy image of this pin will take its position beside that of the far pin. This it will generally do slowly, because the image in the surface might have resulted from a pin of due dimensions situated at every intermediate point, but in continuous succession giving up every intermediate position, it will finally come to rest in its true visible place. Occasionally, owing to the dominancy of some habit, it will seem to take a place at a greater or less distance than the true.

But habits created by magnitude may cause mistakes of visible distance involving also *the image in the axis*.

Thus, for example, if the mind has been accustomed to appreciate the images of mountains of moderate magnitudes, thereby tendencies will be impressed upon it to appreciate moderate dimensions with images of this class. Hence, when the image of a mountain of extraordinary magnitude is presented (in an ordinary state of the atmosphere), consciousness will be influenced to appreciate the dimensions as less than they really are, and, in order to this, the distance from the base of vision as a duly less one. And the effect on any concomitant images, as of trees or animals, would be a like seeming diminution of their visible magnitudes. For it is plain that if consciousness makes a mistake of distance, then, in obedience to the law of habit, it will assign that size which the image would really have at that distance, or that effect of lateral distance, or resulting expansion, which the divergence of the lines of direction would give at that distance.

Dr. Abercrombie relates the following instance as having occurred in his own experience. "I remember," he says, "once having occasion to pass along Ludgate Hill when the great door of St. Paul's was open and several persons were standing in it. They appeared to be very little children; but, on coming up to them, were found to be full-grown persons."

The theory which I have thus advanced, taken in connexion with the mistaking practice of consciousness, will, I believe, furnish a satisfactory explanation of all the phenomena of vision, some of the more interesting of which phenomena, as well as the subject of monocular vision, I propose in a future paper to consider.

Hastings-upon-Hudson, New York.

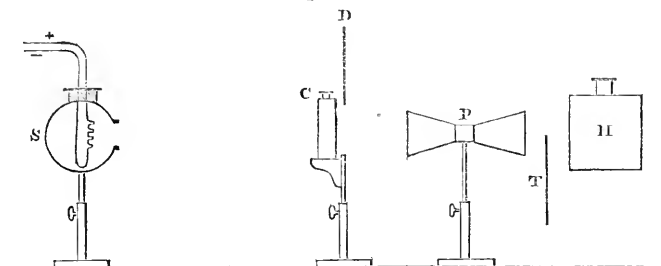
XXVII. *On Sources of Error in Determinations of the Absorption of Heat by Liquids.* By W. FLETCHER BARRETT, *Lecturer on Physical Science at the International College, &c.**

DURING the autumn of 1865, whilst engaged in determining for Professor Tyndall the absorption of heat by various liquids, I observed that under certain conditions the more diathermic liquids exhibited a remarkable and anomalous deportment towards radiant heat. This observation led me to make a subsequent investigation, the results of which are given in the following paper.

* Communicated by the Author, having been read at the British Association, August, 1868.

The general outline of the apparatus employed is shown in fig. 1. The instruments were Professor Tyndall's, who kindly

Fig. 1.



allowed me their use, and also opportunity for making the experiments. The source of heat, *S*, was a platinum spiral raised to bright incandescence by an electric current, and surrounded by a glass globe having an aperture in front. Before this was a cell, *C*, with moveable rock-salt sides to contain the liquid under examination. Before this, again, and precisely opposite, was a thermo-electric pile, *P*, fitted with its conical reflector and attached to a very delicate galvanometer. The rock-salt cell was supported on a little shelf behind a perforated metal screen, *D*, from which the pile was distant some 18 inches. The rock-salt walls of the cell, in the experiments first to be described, were separated by a partition formed of an annular plate of mica 0.02 inch ($\frac{1}{2}$ millimetre) thick: this thickness determined that of the liquid layer under examination. The heat from the spiral was sometimes concentrated by the use of a rock-salt lens (not shown in the figure) which was placed between the source and the cell. To obtain greater sensitiveness, in most of the experiments the "compensating" arrangement was adopted. A blackened canister, *H*, filled with boiling water, was placed before the opposite face of the pile. By adjusting a screen, *T*, between the canister and the pile, the deflection of the galvanometer, caused by the radiation from *S*, could be exactly neutralized. The needle was thus brought to zero and maintained in its most sensitive position.

The empty cell having been mounted on its support in the path of the rays, the screen was moved until the needle stood precisely at zero. A liquid (*bisulphide of carbon*) was now poured into the cell through a fine funnel, and the needle of the galvanometer immediately observed through a telescope. Instead of moving in the direction of absorption, a marked deflection took place on the opposite side of zero, the needle finally coming to rest at 15° on the side of increased heat from the

source; that is, the introduction into the cell of the bisulphide of carbon *augmented* the heat falling on the thermo-pile. When the source of heat was cut off by a metal screen, the needle moved swiftly back to zero and then rapidly up to 67° on the opposite side. From this deflection the absolute increment in the transmission caused by the liquid could at once be calculated. And the mean of eight closely concordant experiments showed that when bisulphide of carbon was poured into a rock-salt cell, so as to form a layer $\frac{2}{100}$ of an inch thick, 9 per cent. more heat reached the pile through such a layer than through the empty cell.

Before passing on to further experiments, I will reply at once to two inquiries which may suggest themselves in explanation of this phenomenon. The one is that the temperature of the bisulphide of carbon might be higher than that of the cell, and that hence its introduction would produce the effect noticed. The other is that the exterior surfaces of the plates of rock-salt might be partially rounded by the frequent polishing which they receive, so that when the bisulphide of carbon was introduced the combination might have acted as a liquid lens. But direct experiments instituted for the purpose of testing these and other possible experimental errors show that they play no part in the phenomenon observed.

Other liquids beside bisulphide of carbon were now tried in layers of equal tenuity; and I was surprised to find a liquid which even surpassed the bisulphide of carbon in this apparently anomalous action on radiant heat. This was the case with *bichloride of carbon*. When this liquid was poured into the rock-salt cell so as to fill the space $\cdot 02$ inch thick, the needle moved from zero to 16° on the side of heat—equal to an increased transmission of 12·4 per cent. At the same thickness bisulphide of carbon increased the transmission 9 per cent. But the fact has now, I believe, been well established that the bichloride of carbon is the most diathermic liquid as yet known. Of *chloroform*, a liquid considerably less diathermic than bisulphide of carbon, the same film of half a millimetre gave an increased transmission of 4·5 per cent. With this liquid (among those examined at this thinness) the effect ceased; for *benzole*, a more powerful absorbent than chloroform, gave an *absorption* of 20 per cent. Of *Sulphuric ether*, a still more energetic absorbent, the same thin layer of half a millimetre absorbed 30 per cent. of the total emission from a red-hot spiral.

These experiments show that, in passing from feeble to more powerful absorbents, the increased transmission, most strikingly observed in the case of bichloride of carbon, becomes more and more disguised until finally it disappears and absorption predominates. A similar effect, as might be anticipated, takes place

by increasing the thickness of a diathermic liquid. With chloroform the addition of $\frac{1}{100}$ of an inch ($\frac{1}{4}$ of a millimetre) to the thickness of the liquid film changed the increased transmission of $4\frac{1}{2}$ per cent., that it previously gave, to an absorption of 8 per cent. In order to follow this change more closely, and to observe where the reversal takes place, I made a series of experiments with bichloride of carbon, gradually augmenting the thickness of the liquid layer until absorption became marked. These results I state at once in a tabular form. For the sake of clearness and brevity I have omitted the deflections, which gradually became less till zero was crossed, when they then increased on the side indicating absorption. Taking the transmission through the empty cell as equal to 100, the figures in the second column show the transmission by the layers of liquid of the thickness given in the first.

Radiation through Bichloride of Carbon enclosed between Rock-salt plates.

Source:—platinum spiral at a low white heat.

Thickness of liquid layer in parts of an inch.	Transmission per 100.
0.02	112.4
0.03	107.0
0.04	105.3
0.11	100.8
0.14	100.0
0.27	94.0

The singular fact is here brought out, that at a thickness of .14 of an inch the transmission through the bichloride was the same as through the empty cell. Eighteen months after this some of these determinations were repeated, and no appreciable difference from the above Table was discovered.

In connexion with past and future determinations of the absorption of heat by liquids, I need hardly point out the influence of the fact here established, and the conclusions to which I shall allude in the sequel.

The foregoing experiments were made with a cell having *rock-salt* ends, and they show that *absorption* becomes manifest after a certain thickness in a diathermic liquid is obtained. This is not the case, however, when glass instead of rock-salt cells are employed to contain the same liquids: here the anomaly is still more striking.

The first experiment in the next series was made with a cell having plane and parallel glass ends 1.2 inch diameter. Representing by 100 the transmission through the empty glass cell,

filling the cell with bisulphide of carbon increased this number to 106.

The experiment was twice repeated with the same result; and the effect was not uncertain or transitory, but a steady permanent augmentation.

I next tried a series of cells with parallel glass ends of varying distances asunder, ranging from 0.1 of an inch to 3 inches, bisulphide of carbon being used in each case. The source and the disposition of the apparatus were the same throughout, and the result of all the experiments showed that increasing the depth of the cell did not, as was the case with rock-salt cells, reverse the effect, but rather augmented the increased transmission—the transmission through an empty glass cell 3 inches deep being raised 9 per cent. by filling it with bisulphide of carbon.

Returning to rock-salt plates, for the sake of comparison a cell 2 inches deep was tried. As anticipated, instead of increased transmission, considerable absorption was now observed when the bisulphide of carbon was poured into the cell. Another similar rock-salt cell, 0.8 of an inch deep, was also tried, with the same result. In fine, with cells having rock-salt ends, after a thickness of 0.2 of an inch had been attained, absorption invariably was found on the introduction of bisulphide and bichloride of carbon; but *with cells having glass ends, augmented transmission was always observed with both liquids*, the bichloride of carbon having always a slight advantage.

There can therefore be no doubt about the fact that the absorption of the more diathermic liquids is entirely disguised when glass cells and a source of rays of the character here used are employed in their examination. Moreover the effect was found to remain the same when the nature of the source was altered, or its temperature raised or lowered, or when the cell was moved nearer to or further from the source of heat: nor was the result changed by causing the diaphragm in the screen (D, fig. 1) to be larger or smaller than the diameter of the aperture in the cell. The same result was also obtained whenever the experiments were made. For example, I here collate a number of determinations, some made at an interval of twelve months apart. The radiation through an empty glass cell 1.2 inch deep being taken as 100, the numbers

106.2, 106.9, 104.6, 106, 106,

or a mean of 6 per cent., expressed the increased temperature noticed when the cell was filled with bisulphide of carbon. Nevertheless the result is just as certain that when the walls of the cell are of rock-salt this increase is not shown; numerous experi-

ments, which it would be tedious to record, have established this fact.

Although the comparison between the rock-salt and glass cells was attended with so uniform a result, yet it is open to the objection that the walls of the two kinds of cells were of different thicknesses; for, from its friability, it is necessary to use rather thick plates of rock-salt. A stricter comparison was therefore instituted. Two plates of glass of exactly the same thickness as the plates of rock-salt, 0·3 of an inch, were cut from a slab of the so-called patent plate—a glass perfectly colourless when looked at edgewise. Two thinner plates, 0·1 of an inch, of the same glass were also procured to compare with the thick plates. The same moveable cell was employed for both the rock-salt and the glass, so that in both cases the diameter of the cell and the position which determined the depth of the liquid layer were the same. The source of heat was the same as in the former experiments, and nothing intervened between it and the cell. The conditions of the experiment being thus exact, I sought at the same time to determine more precisely the difference between the deportment of bisulphide and bichloride of carbon. The results of these experiments are given in the following Table:—

Source :—bright-red platinum spiral.

Layer of liquid $\frac{1}{10}$ of an inch thick.

	Free radiation through	Transmission.
I.	Empty cell, thin glass plates . . .	100·0
	Filled with bisulphide of carbon . . .	103·5
	Filled with bichloride of carbon . . .	104·0
II.	Empty cell, thick glass plates . . .	100·0
	Filled with bisulphide of carbon . . .	109·5
	Filled with bichloride of carbon . . .	111·2
III.	Empty cell, rock-salt plates . . .	100·0
	Filled with bisulphide of carbon . . .	97·5
	Filled with bichloride of carbon . . .	101·5

Here, the liquid layer remaining the same, the *thick* glass plates, it will be seen, instead of reversing the action, actually increase the transmission 6 per cent. over and above that which occurs when *thin* glass plates are used. The objection alluded to is thus not only removed, but the above result afforded a clue by which a solution of the enigma was finally obtained. The bichloride of carbon, it will also be observed, under all circumstances surpasses the bisulphide in augmenting the transmission.

An interesting result occurred in the course of the experiment with the glass cells. In place of the transparent bisulphide and bichloride of carbon, I tried the same liquids saturated with

iodine and thus rendered opaque. Notwithstanding the augmented absorption by the iodine, I still found a larger amount of heat fall on the pile in both cases on pouring the opaque liquid into the cell. When a glass cell $\frac{1}{2}$ an inch deep was used, the heat falling on the pile was raised 3 per cent. by the opaque liquid. When a glass cell 1·2 inch deep was employed, a similar increase of fully 5 per cent. was produced, and this by the introduction of a liquid so opaque that at this thickness it intercepted all the light of the brightest sun.

What, now, is the explanation of this singular action? Reviewing the experiments detailed in the foregoing paper, they are seen to divide themselves into two groups:—

First. Those where an increased transmission attended the introduction of moderately diathermic liquid *films* between plates either of rock-salt or of glass; and

Second. Those where this increase occurred with considerable thicknesses of the same liquids as before, but only when the liquids were enclosed between *glass* plates.

The most probable explanation of the first group of facts at once suggests itself—namely, that the introduction of the liquids diminishes the reflection taking place from the inner surfaces of the walls of the cell. In the empty cell a double reflection takes place when a pencil of rays passes from one plate of the cell to the other through the intervening space of air. If the cell be filled with a liquid whose refractive index is the same as that of the walls of the cell, this loss by reflection from the inner surfaces will be abolished; hence such a liquid, if its absorbent power were *nil*, or extremely low, would increase the transmission when replacing air in the interior of the cell. In the following Table are given the refractive indices of the substances employed; as these were not determined from the specimens used, they can only be regarded as approximately correct; that of glass is especially doubtful. The indices for rock-salt and bichloride of carbon were kindly furnished me by Dr. Gladstone, and are from his own recent determinations:—

Index of Refraction for the line A.

Plate-glass (mean)	1·525
Rock-salt	1·537
Bisulphide of carbon (mean) .	1·646
Bichloride of carbon	1·456
Chloroform	1·441

From this Table it will be seen that the index of refraction of bichloride and bisulphide of carbon is sufficiently near to rock-salt to greatly diminish the reflection from the interior of the

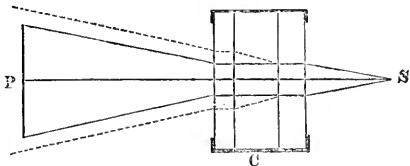
cell, whilst, further, bichloride and bisulphide of carbon are two of the most diathermic liquids known. The index of the bichloride is nearer that of glass and rock-salt than the index of the bisulphide, just as might be expected. Melloni has stated that 7·7 per cent. of the incident rays are reflected from a single polished plate of rock-salt*. Although this figure is probably rather too high, still there can be no doubt that any moderately transcendent liquid, when poured into a cell whose walls *were sufficiently near together*, would, by its optical density, increase the transmission more than it would intercept it. And this is exactly what has been found in the experiments described in the early part of this paper. As, moreover, this abolition of reflection of the inner surfaces of the cell will take place at all thicknesses of the liquid layer, the absorption hitherto attributed to the majority of liquids must be too low on account of this opposing effect—an effect as yet disregarded, but here shown to be very sensible.

The foregoing explanation, however, is quite incompetent to explain the second group of facts—the augmented heat observed when certain liquids were enclosed in vessels with glass ends. For here the augmentation continued until the thickness of the liquid was increased to the limits of experiment; and in this case the loss from absorption by the liquid must have been greater than the gain, even if the interior reflection had been entirely abolished.

Here the cause is, I believe, mainly due to a well-known effect of plane surfaces on the refraction of divergent rays. This will be clear if we consider the case represented in fig. 2. Let S be

a source of rays diverging till they meet the cell C, and let P be the surface of the thermo-pile. If, now, the vessel C be empty, the rays, after passing through its sides, will move in the direction of

Fig. 2.



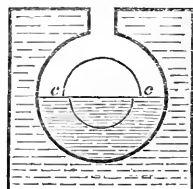
the dotted lines. Now let this vessel be filled with a liquid whose density is approximately the same as the walls of the cell, the rays will then take a course indicated by the continuous lines. In the latter case the angle of the rays emerging from the cell will be more acute. The distance of the divergent point S from the pile P has thus virtually been diminished, and, in consequence, many rays which, when the cell was empty, escaped the pile, now fall on its surface. To obtain this action it is not, of course, necessary that the refractive power of the liquid should be the same

* *La Thermochrose*, p. 195.

as the walls of the cell; for a similar effect will take place, in a greater or less degree, if the density of the body introduced exceed that of the medium it replaces.

It was easy to see the truth of this reasoning by tracking the course of a pencil of luminous rays through the empty and full cell. The light of the red-hot spiral that formed the source of heat was received on a semitransparent screen placed behind a glass cell half an inch deep. The beam was seen on the screen as a luminous circle 1.7 inch in diameter. Filling the cell with bisulphide of carbon, the diameter of this circle contracted to 1.6 inch. When the cell was half full, the effect of the comparison between the upper and lower half of the circle was very striking; this is shown in fig. 3. With the cell 3 inches deep the luminous circle *cc* was diminished from 2.4 inches, when the cell was empty, to 1.8 inch, when it was filled with bisulphide of carbon.

Fig. 3.



This observation explains the effect noticed when bisulphide or bichloride of carbon was poured into deep glass cells; for though sometimes concentrated by a rock-salt lens, the rays from the source were always more or less divergent, and overlapped the area of the pile. Lessening the divergency of the incident rays ought to diminish the effect; and this, by experiment, was proved to be the case. Hence also with *parallel* rays no concentration of the beam could take place on the introduction of the liquid, and with *convergent* rays a decreased amount of heat should reach the pile when a dense medium occupies the cell.

But why should not an increase occur with rock-salt as well as with glass cells? This anomaly puzzled me a good deal at first; for, on consulting the Table of refractive powers, there does not appear to be any reason why such a difference should exist. I think, however, the answer to this question is mainly to be found in the difference between the diathermancy of rock-salt and glass. Rays of heat pass through the former substance with but little alteration in their quality or quantity; but in passing through the latter, their character is changed as well as their amount lessened; after passing through one plate of glass they traverse a second plate with less absorption. It may be that the bisulphide of carbon absorbs somewhat the same class of thermal rays as glass; the heat-tint or thermochrose of the two bodies is possibly alike. Assuming this to be the case, when a cell with glass sides is used, the absorption of the liquid would be so far reduced that the action of the causes tending to increase the transmission would become evident. On the other hand, with a

rock-salt cell the heat-rays would not be sifted in any sensible degree, and hence the absorption by the deep layers of liquid which were employed would be sufficient entirely to mask the opposite effect observed with a glass cell. I regret that when making these experiments it did not occur to me to try how far the foregoing assumption is true, by causing the beam to pass through a plate of glass before it entered the rock-salt cell; and now I have not the opportunity of making this crucial test. But the following experiment, which strikingly shows the influence of sifting, may be taken in support of the view here stated. The divergent rays direct from the source of heat used throughout these experiments were caused to pass through an empty glass cell $\frac{1}{2}$ an inch deep. Falling on the pile, they gave a deflection represented by 100. Filling the cell with bisulphide of carbon augmented this number to 110, or an increased heat of 10 per cent. The cell was emptied, replaced, and a second cell 3 inches deep, already filled with bisulphide of carbon, was interposed in the path of the rays from the source. The beam had thus to traverse 3 inches of bisulphide of carbon before it reached the empty cell, so that on passing through an additional $\frac{1}{2}$ inch of the same liquid, but little or no further absorption would take place; and the action of the liquid in concentrating the rays should thus be more pronounced. Representing by 100 the radiation through the 3-inch stratum of liquid and the empty $\frac{1}{2}$ -inch cell, filling the latter with bisulphide of carbon raised this figure to 120, or an increased heat of 20 per cent. Compared with the former observation, this experiment shows that even in a glass cell at least 10 per cent. of the total radiation is absorbed by a layer $\frac{1}{2}$ an inch thick of bisulphide of carbon; much more likely, therefore, is it that in a rock-salt cell the absorption by deep layers of liquid should disguise the causes tending to augment the transmission.

Finally, it may be asked if pouring the bisulphide of carbon into a glass cell increases the heat reaching the pile, how is it that Melloni, who, as is well known, used glass cells in his experiments on the diathermancy of liquids, does not mention the fact? This is accounted for, I believe, by Melloni's method of experiment: he did not determine the transmission through his empty glass cell first, and then through the liquid added to the cell; but noting only the free radiation to his pile, he then introduced the cell and liquid at once. Consequently he assigned an absorption to some liquids which probably was wholly due to the glass walls of his cell. Hence it is that whilst Melloni states a layer of bisulphide of carbon $\frac{1}{2}$ inch deep absorbs 37 per cent. of the heat from an argand lamp, Dr. Tyndall, who used a rock-salt cell and

a better method of experiment, finds the absorption by a corresponding thickness of liquid under 20 per cent.*

Summing up the results of this inquiry, the main points are as follows. :—When the more diathermic liquids are introduced between two plane parallel plates of rock-salt separated by a very small interval, the rays from an artificial source are found to be more freely transmitted than when air intervenes between the plates. For a space $\cdot 02$ inch wide the increased transmission amounts with bichloride of carbon to about 12 per cent., with bisulphide of carbon to 9 per cent., and with chloroform to 4·5 per cent. This effect disappears and less heat is transmitted (1) when the transparency of the liquid diminishes; *e. g.* the same thickness of sulphuric ether intercepts 30 per cent. of the heat previously passing through the empty cell; (2) when the distance between the plates is increased beyond, say, $\frac{1}{10}$ of an inch in the case of bisulphide, and $\frac{2}{10}$ of an inch in the case of bichloride of carbon. The increased transmission by these liquids reappears, however, in thicker layers when plane parallel *glass* plates are substituted for rock-salt, and continues, apparently indeed augmenting, as the depth of the cell increases, so far as the experiments were carried. Bisulphide of carbon, poured into a cell with glass sides 1·2 inch apart, increases the heat falling on the pile 6 per cent., and bichloride of carbon a still larger amount. Altering the temperature or nature of the source, the size of the aperture in a screen behind the cell, or the position of the cell, makes no material change in these results. But altering the character, or augmenting the thickness, of the walls of the cell has considerable influence. For example, if the cell-walls be of glass, increasing their thickness from one- to three-tenths of an inch raises the heat falling on the thermoscope 6 per cent. when equal depths of the selfsame liquid are poured into the cell. Again, by merely changing the parallel sides of the same cell from rock-salt to precisely similar plates of glass, the very same liquid can be shown to intercept a certain quantity of the heat falling on the thermo-pile in the one case, and to augment that quantity in the other—the difference amounting to upwards of 10 per cent. of the total radiation through the empty cell.

* Dr. Tyndall has pointed out (Phil. Trans. 1864, p. 328; Phil. Mag. vol. xxviii. p. 439) how seriously the glass cells used by Melloni must have influenced the results of his experiments. Melloni, indeed, saw this himself, but endeavoured to prove, by trying two of his liquids between rock-salt plates, that the error vanished in the thickness of liquid he employed; (*La Thermochrose*, pp. 163 and 199). The liquids chosen were colza oil and water—the worst he could have selected; for had Melloni made the comparison with more diathermic liquids, he would have discovered his error.

The explanation of the foregoing facts may be traced to two main causes. The increased transmission noticed with *films* of the more diathermic liquids chiefly arises from the reduction or abolition of the reflection taking place from the interior surfaces of the walls of the cell, owing to the optical density of the liquid introduced being nearer to the cell-walls' than that of the medium it replaces. But in glass cells of considerable depth, retaining the former explanation, the augmented heat there observed is probably *mainly* due to an effect of the refraction of divergent rays by plane surfaces; this gives rise to a concentration of the beam, which becomes sensible when accompanied by a great transealency of the liquid in the cell. In similar cells with rock-salt ends the effect is not observed, probably on account of such cells sifting the beam far less than glass, and thus permitting a higher absorption of the liquid. Nevertheless even with rock-salt cells the causes alluded to must necessarily render, to a certain extent, incorrect the precise absorption hitherto attributed to liquids. These sources of error in determining the true absorption by a liquid or solid can, however, be avoided by employing truly parallel rays; and these are best obtained from the sun.

In conclusion I would add that I do not pretend the observations contained in the foregoing paper can in any material way affect any portion of the celebrated researches on radiant heat which Professor Tyndall has so long conducted. On the contrary, to my mind, they add lustre to that research, by showing in some degree the difficult and delicate nature of the inquiry. In fact this little investigation of mine is not of such importance as to merit even this reference, were it not that I speak from the sincere and jealous attachment of a disciple to his master.

XXVIII. *On a Simple Method of Exhibiting the Combination of Rectangular Vibrations.* By W. FLETCHER BARRETT, Lecturer on Physical Science at the London International College, &c.*

PHYSICISTS are well acquainted with the elegant experiments of M. Lissajous, in which the vibrations of two tuning-forks, placed at rectangles, are optically combined by viewing a ray of light successively reflected from a mirror attached to each fork. A regular series of curves is thus obtained which give a perfect optical expression to each of the musical intervals, the curves augmenting in complexity as the dissonance between the forks increases.

* Communicated by the author, having been read at the British Association, August, 1868.

Instructive and beautiful as are these experiments, the extreme costliness of the apparatus necessary for their proper exhibition has hitherto debarred many from repeating them.

A more simple method of combining rectangular vibrations was long ago devised by Mr. Wheatstone, who employed for this purpose a slender prismatic rod of steel, fixed at one end, and free to vibrate at the other. One of the sides of the rod was filed away, until the vibrations were quicker in one direction than the other by a certain definite ratio. When pulled aside the rod compounded these vibrations, and the resultant motion became apparent by the movement of a silvered bead attached to the free extremity of the rod. The labour of filing away the steel, and the necessity of having a separate rod for each combination, has practically prevented this instrument coming into use. Beyond a most ingenious mechanical contrivance for representing the combination of vibrations, I am not aware that Mr. Wheatstone has designed any simple instrument for effecting this purpose*.

Upwards of two years ago I found a method of obtaining any desired combination by an extremely simple arrangement. A piece of straightened steel wire, about No. 16 gauge and some 12 or 18 inches long, is first well softened in a flame at a point 6 or 8 inches from the end, which length is then bent downwards. The extremity of the longer portion is fixed in a vice, a silvered bead is cemented by marine glue on to the summit of the bend, and the instrument is complete. The whole system is thrown into vibration by smartly tapping the wire near the point held in the vice, and *in a direction oblique to the plane of the two wires*. The vibration travels up the wire, rounds the bend, and throws the inclined arm into motion. The latter, being free, vibrates more easily than the portion which is fixed at one extremity; a compound motion is thus the result, and the spot of light, reflected from the bead, describes a curve expressing the resultant action.

The ratio between the vibrations of the two parts of the wire can evidently be adjusted, or altered, by raising or lowering the point clamped in the vice. The same end may also be obtained by loading the free portion of the wire by a little sliding weight. Both these means of adjustment I have tried with some success. I was not, however, prepared to find that an alteration in the angle of the bent wire would yield a more satisfactory result; but so it is. When the wires are parallel† and even in length, a

* May I here be allowed to express a wish that Mr. Wheatstone would collect and republish his numerous papers? So scattered are they, that I have searched in vain for the original description of the *Kaleidophone*.

† That is, nearly parallel, which is obtained by making a round bend.

combination of 1 to 1 is obtained, and the bead describes a circle passing into an oblique line; but on opening the free limb to an angle of about 30° , the figure changes into the complex curve given by the ratio of 4 to 5. Opening the angle still further, the curve expressing the ratio of 3 to 4 is obtained; then at 45° 2 to 3; and at an angle of 75° the figure of 8 comes out, expressing the ratio of 1 to 2. In fact, by varying the angle an entire series of combinations, more or less perfect, can be produced at will.

In order to avoid the probable breaking of the wire by repeated bendings, I had a light hinge joint made at the bend; but it was less satisfactory than the simple bent wire, which, if well annealed, will bear innumerable bendings, and can be renewed in a moment if broken. To obtain a steady figure, it is better to make the final adjustment by slightly raising or depressing the fixed wire.

Fig. 1.



Fig. 2.



Figure 1 shows the instrument. The wire is capable of being *firmly* fixed at any height in a support which is attached to a heavy stand, more convenient in use than a vice.

Not only may this arrangement be used for exhibiting the combination of vibrations, but it also shows very prettily the formation of nodes and ventral segments. Placing a piece of white paper behind the instrument, or casting its shadow on a screen, the vibration of the wire may be distinctly seen by a great number of people. The fixed arm will then be noticed always to vibrate as a whole, and the bend always to be a ventral segment.

But on the free arm an instructive change is seen to take place in the position of a node which is there formed. When the arms are equal and parallel, and a ratio of 1 to 1 obtained, the

node is near the free extremity of the bent wire; as the wire is raised and the angle increases, the node rises nearer to the bend. It is also worth observing that, in any combination, the distance of the node from the free extremity of the wire, compared with its distance from the bend, is approximately the same as the ratio of the interval depicted by the figure.

I have now, in conclusion, to refer to another arrangement for effecting the combination of rectangular vibrations, which though rather less simple than the one just described, has the merit of being more easily adjusted and more permanent in character. This arrangement, shown in figure 2, has been adapted by Mr. Ladd from an instrument devised by Professor Helmholtz.

Two flat pieces of steel are here welded at right angles to each other into a single rod. The upper part (*a*) is tapering, and on its summit is fixed a polished silver bead. The lower part (*b*) is capable of being firmly fixed in a suitable support. According to the height at which *b* is clamped, so a corresponding portion is allowed to enter into vibration. A combination of the vibration of *a* with that of *b* can thus be obtained in any given ratio. Complete command of any figure can be had by marking its position on the lower strip of steel; and so nice an adjustment is possible, that an almost absolutely steady figure can be secured with a little care.

If I may presume to suggest a name, I propose to call the instrument described in this paper a *Tonophant*.

XXIX. Notices respecting New Books.

A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science.
By CHARLES W. ELIOT and FRANK H. STORER. Second Edition.
London: Van Voorst. (Pp. 605 and lix.)

AT a time when the attempt has become general in this country to elevate natural science to the rank of a branch of education, even the minutest incident of the transition may be considered important. If report be correct, we are about to be rescued from the hands of the popular lecturer and placed under the regimen of the schoolmaster; and even those by whom the advantages and functions of science are but little understood manifest an earnest interest in every symptom of the change.

The number of chemical manuals which have been published during the last three years, for the most part somewhat hastily, are a sufficient proof that chemists expect a new epoch in national education. We have now in common circulation thrice the average amount, perhaps, of such works; and new ones, as well as new editions of old ones, are announced as preparing for the press. Most of these books are literary experiments, and it is very unlikely that more than one or two of them will achieve a lasting reputation. Not

unfrequently an author, unable to guess what will be the forthcoming educational plan, writes without a definite idea of any kind to guide him, unless it be that of addressing some particular class of students at a given school; and if one insists wholly on the inductive method, another asserts the necessity of much deductive preface, while a third is lost in transcendental speculation. A great deal of this confusion might have been avoided by assuming the independent attitude and occupying the lofty position of the older writers. At the end of the last and beginning of the present century, for example, it was no doubt the intention of such chemists as Macquer, Lavoisier, Davy, and Thénard to place within the student's reach a manual which should exhibit chemistry as *a whole*; it is, in the main, works of such a nature that were then most extensively read. Even where detail was avoided rather than given, the reader could never have felt the impression from such a book to be fragmentary. He resembled an observer with the microscope viewing an object with a low power. He might see chemical facts brilliantly illuminated by inductive light, deduction proffered by a master mind, and, naming (according to custom) every chemist a philosopher, might perhaps imagine himself a witness of the golden age. The subsequent progress of science has, indeed, pointed out deficiencies in that epoch; but it has never made a general improvement on the foremost manuals which then appeared. They still remain as models of completeness; and a strong deductive tendency (sometimes latent, sometimes in bold relief) is one of the noblest characteristics they display.

The manual of Professors Eliot and Storer is evidently intended chiefly for laboratory use, and is designedly much more practical than theoretical in its tone. With its aid, a teacher might possibly dispense with lectures to a great extent—a course which the book itself may almost be said to suggest. The authors seem to use it as an aid to “classes in the laboratory;” and, from the appearance of two editions in the course of one year, it may be inferred that their plan has been favourably received in American schools. It is highly probable that some such scheme will be adopted among ourselves if chemistry really becomes an element of our national education. It would indeed be a misfortune if the lecture system should be wholly abolished; for we should thereby lose one of the principal means of retaining pure science among us. But good lecturers will never be numerous, certainly never in proportion to the general demand in the case supposed. Most of the students will be committed to the care of comparatively young instructors, whose slight theoretical attainments will inspire respect much more slowly than their practical gifts. Under these circumstances chemistry, if it is to be taught at all, will have to be taught partially—that is, for the most part in a practical way; and some work like the present will doubtless be in general use.

While decidedly of opinion that the partial teaching of a subject is rather a mitigated evil than an undoubted blessing, we have noticed with pleasure or interest that, of the very few chemical manuals which make strong inductive professions, not one can escape

the charge of being somewhat deductive or speculative. A well-marked instance appears in the present work. The following passage occurs at p. 605 :—"The existence of atoms is itself an hypothesis, and not a probable one; all speculations based on this hypothesis, all names which have grown up with it, all ideas which would be dead without it, should be accepted by the student provisionally and cautiously, as being matter for belief but not for knowledge." Yet the entire volume is provided with the dicta and terminology of the atomic theory, and even contains discussions of "true molecular formulæ." Such an inconsistency (or perhaps inadvertence) is the more to be regretted, since any allusion to the atomic theory in the systematic teaching of chemistry is quite unnecessary. The authors themselves evidently regard Daltonism with disfavour; and it is to be hoped that, for ordinary school-purposes, it may ere long cease to be regarded at all.

The matter of this work gives us great satisfaction. It is well arranged, drawn from good sources, and very correct. Many of the experimental illustrations are new, and upon all of them that we have examined a large amount of judgment and thoughtfulness have been expended: they are, moreover, sufficiently numerous for a lengthy course of instruction. Professors Eliot and Storer have therefore achieved, and very well achieved, the object they had in view. Their manual would no doubt have read more agreeably if the Berzelian nomenclature had been employed and multiplied formulæ (such as $\text{H}^2\text{N}^2\text{O}^6$ for hydric nitrate) had been omitted; but these may be matters of local and temporary expediency, which time, we trust, will remove. We should also recommend the enlargement of the appendix (on Chemical Manipulation), its present dimensions being somewhat disproportionately small.

The publication of an American manual of chemistry in England is a rare circumstance, and would in itself be of interest to our readers; but the intrinsic merits of the book constitute a more legitimate claim on their attention. Its unique character will also commend it to the perusal of every one whom the results of the present educational crisis may nearly affect.

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

"On Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S.

This memoir is divided into six parts. The *first* part contains a definition of the subject; the *second* an historical sketch; the *third* is on the action of *nuclei* in inducing crystallization, and the effect of low temperatures on a number of supersaturated solutions contained in chemically clean vessels; the *fourth* is on the formation of

a modified salt, as in the case of zinco-sulphate and sodic sulphate; the *fifth* contains an inquiry as to whether anhydrous salts form supersaturated solutions; and the *sixth* and last part is a summary, with a classified list of the salts examined.

1. *Definition*.—When water at a high temperature is saturated with a salt, and, on being left to cool in a closed vessel, retains in solution a larger quantity of the salt than it could take up at the reduced temperature, the solution is said to be supersaturated.

2. *History*.—During many years the phenomena of supersaturation were studied with reference to solutions of Glauber's salt. In 1809, Ziz of Mayence* showed that the sudden crystallization of these solutions is not due to agitation; that the vessels containing the solutions do not require to be hermetically sealed, but if put under a bell-glass, or loosely covered (as with a capsule), they can be preserved during a long time; that solids brought into contact with the solutions act as *nuclei* and produce instant crystallization, but that such solids act best as nuclei when dry; if wet or boiled up with the solution, they become *inactive*. The most efficient nucleus is a crystal of the salt itself. Air, if artificially dried, ceases to be a nucleus. Three varieties of the sodic sulphate are noticed, *i. e.* the *anhydrous* and the ordinary *10-atom hydrate*, and also a peculiar salt formed when supersaturated solutions in closed vessels are left to cool down. This *x* salt, as it is termed, contains less water of crystallization than the ordinary salt, and is more soluble. If the vessel in which it is formed be suddenly opened, or the mother-liquor touched with a nucleus, the mother-liquor instantly solidifies into the 10-atom hydrate, and the *x* crystals become opaque, like the boiled white of an egg.

In 1819 Gay-Lussac† referred the state of supersaturation to the inertia of the saline molecules, the molecular condition of the sides of the vessel, and other causes. He also showed that solutions of some other salts exhibit the phenomena of supersaturation. In 1832 the number of such salts was shown by Dr. Ogden‡ to be not less than twenty-one.

In 1825 Faraday§ published some experiments on the supersaturated solutions of Glauber's salt. Graham||, Turner¶, Ure, and others also contributed new facts; but the most elaborate inquiry was by M. Löwel between the years 1850 and 1857, the results of which are contained in six memoirs**. According to this writer, the ordinary 10-atom sodic sulphate increases in solubility from 32° to 93°·2 F., at which latter temperature it begins to fuse in its water of crystallization, and to deposit the anhydrous salt. This salt follows an inverse order of solubility as compared with the 10-atom hydrate, its solubility diminishing as the temperature rises; or, what is the

* Schweigger's Journal, 1815, vol. xv.

† Annales de Chimie et de Physique, 2nd ser. vol. xi.

‡ Edinb. New Phil. Journ.

§ Quarterly Journal of Science, vol. xix.

|| Trans. Roy. Soc. Edinb.

¶ Elements of Chemistry.

** Annales de Chimie et de Physique, 3rd ser. vols. xxix., xxxiii., xxxvii., xliii., xlv., xlix.

same thing, from 218° , the boiling-point of a saturated solution, down to 64° the solubility increases; but at 64° the solution undergoes a new molecular modification, and begins to form crystals of the 7-atom hydrate (the x salt of Ziz). This salt is much more soluble at ordinary temperatures than the 10-atom hydrate, its maximum solubility being at $80^{\circ}6$. Thus the sodic sulphate has three maxima of solubility, viz. $93^{\circ}2$ when it is under the molecular constitution of the 10-atom hydrate, $78^{\circ}8$ to $80^{\circ}6$ when it is under the molecular constitution of the 7-atom hydrate, and $62^{\circ}6$ to $64^{\circ}4$ when it is under the molecular constitution of the anhydrous salt. At these three maxima the saturated solutions are about equally rich in salt. The 7-atom hydrate and the anhydrous salt can only maintain their molecular constitution when in contact with the mother-liquor in closed vessels, in which they are sheltered from the air and from other bodies that act upon them as nuclei. No sooner are they exposed to the air than they become opaque and warm, and assume the molecular constitution of the 10-atom hydrate, as well as its solubility. Hence the conclusion is that supersaturated solutions of the sodic sulphate are not really so, since they hold a salt of much greater solubility at ordinary temperatures than the normal 10-atom salt. Löwel extends his inquiry to sodic carbonate and magnesia sulphate, and endeavours to show that in their supersaturated solutions salts of a lower degree of hydration and of greater solubility than the normal salts are formed; and his general conclusion is that all cases of supersaturation are in appearance only, and not in fact. As to the function of nuclei and the inner sides of the flasks in determining crystallization, he regards it as the effect of one of those mysterious contact actions known as *catalytic*, of which science has not yet been able to give a satisfactory explanation. Bodies that appear to be active in inducing crystallization are designated as *catalytic* or *dynamic*, while bodies that are apparently inactive are termed *non-catalytic* or *adynamic*. "It appears certain," he says, "that but for the mysterious action which the air and other bodies exert on supersaturated solutions, we should obtain sulphate of soda only in the modified state—that is, crystallized with seven equivalents of water, and possessing at ordinary temperatures of the air a much greater solubility than that of the normal 10-atom salt."

Later inquirers have endeavoured to explain the nature of the force exerted by nuclei in inducing crystallization under certain conditions, and their passivity under others. Gernez* tried no less than 220 solids, and of these he selected 39 that were active in inducing crystallization: 18 were insoluble; these were carefully washed in distilled water, and dried out of contact with air. When dry they were found to be without action on the solutions they had previously caused to crystallize. The 21 soluble substances were purified by recrystallization, and they all became inactive. Hence it is concluded that sulphate of soda is the only nucleus for solutions of the same salt. That is to say, whenever a glass rod or other body acts as a nucleus,

* Comptes Rendus, vol. ix. p. 833. A similar method was adopted by Schiff, Ann. der Chem. und Pharm. vol. exi. p. 68.

it is contaminated with minute portions of the salt itself, which M. Gernez believes to exist in the air, not only of towns, but in the country. According to this view, the supersaturated solution of any other salt can only be crystallized by a saline nucleus of its own kind. But, as M. Jeannel* has pointed out, if this theory be true, we must have floating in the air specimens of all kinds of salts that form supersaturated solutions, and crystallize by the introduction of a solid nucleus; whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air. M. Jeannel shows that a few drops of an ordinary solution of a salt will induce crystallization in a supersaturated solution of the same salt without contact of air.

3. *On the Action of Nuclei.*—With respect to the action of nuclei on saline supersaturated solutions generally, the author refers to a theory of his† which seems to account for the liberation of gases from their supersaturated solutions (soda-water, seltzer-water, champagne, &c) when a solid nucleus that had been exposed to the air is immersed in them; while such nucleus becomes inactive if kept long in water, or passed through flame &c., and dried or cooled out of contact with air. The action of nuclei is referred to adhesion. Nuclei are active in inducing crystallization, or they are inactive, according to the state of chemical purity of their surfaces. In the case of a supersaturated saline solution, the sides of the vessel may act as nuclei, or any solid, and some liquid, bodies brought into contact with it. Now suppose the inner surface of the vessel to be made chemically clean, either by well washing it with strong sulphuric acid, or caustic alkali, or spirits of wine, or, as often happens, by boiling the saline solution in the vessel in which it is intended to be kept. In such cases there is perfect adhesion between the sides and the solution, and no salt will be liberated; the sides may in fact be regarded as merely a continuation of the liquid itself, and no salt can be formed there, any more than in the central parts of the liquid. But suppose the sides to be not chemically clean—to be more or less dirty, in fact; in such cases adhesion is diminished or destroyed, and the surface of the liquid next to such sides is virtually as free as its upper surface. Salt will be deposited there, other circumstances being favourable, really from want of adhesion between the side and the liquid that holds the salt in solution. Now apply this to the case of a so-called “adynamic,” “non-catalytic,” or “inactive” glass rod, or coin, or fragment of glass or of flint, &c. A glass rod placed in the solution does nothing more than form new sides, as it were, to the vessel; and its effect is merely that of the sides. If chemically clean, the rod will form no crystals about it, and hence it is “inactive” because its adhesion is perfect. If dirty, the surface of the solution in contact with it will be as free, or almost so, as the upper surface. It requires special means to produce a chemically clean surface; and when produced, it is not easy to maintain it. A short exposure to the air, or a mere touch, will suffice to cover it with an organic film, or with

* Ann. de Chim. et de Phys. 4th ser. vol. vi. p. 166; and Comptes Rendus, Jan. 2, 1866.

† Phil. Mag. July and August 1867.

motes or dust that prevent or lessen adhesion between it and the aqueous part of the solution, and apparently render an inactive solid active. When a glass rod &c. has been kept in water or passed through flame and dried, or cooled out of contact with the air, it is more or less chemically clean, and remains so while sheltered. When Ziz found a knitting-needle active on one solution, and by passing it through the cork which confined a similar solution it became inactive, he simply made the wire chemically clean by the friction. Air is not a nucleus, and when it appears to act as such, it is simply as a carrier of some solid particle not chemically clean. Hence narrow-necked flasks when opened retain their solutions liquid longer than wide-necked ones, as the former are less likely to catch motes &c. from the air than the latter. Supersaturated solutions are best preserved by plugging the necks of the flasks &c. with cotton-wool, since in cooling down the air is filtered in passing through the plug, and motes and dust are thus kept back.

Tubes made chemically clean by the action of strong sulphuric acid may be filled with a strong solution of sodic sulphate, and when cold the tubes may be placed in a freezing-mixture at 10° F. without any separation of the salt. Hence the author differs from M. Löwel's theory, which supposes a molecular change to take place when strong solutions of the salt are cooled down below 60° . Supersaturated solutions of various salts were cooled down to various temperatures from 32° to 0° F. without crystallizing. Sodic acetate, for example, was kept for some hours at 14° , when on touching it with a wire it became solid, and the temperature rose to 104° . Sodic arseniate, sodic succinate, sodic borate, sodio-potassic tartrate, potash alum, and other saline solutions were treated in this way. Some of these solutions become viscid at a low temperature, and do not immediately crystallize on removing the cotton-wool plug. If they be touched, or the side of the flask scratched with a chemically clean wire, there is no action; but if the wire be not chemically clean, the scratches immediately become chalky white by being covered with minute crystals of the salt, and the action then spreads until the solution becomes solid.

Some salts that are not very soluble in water, such as the plumbic acetate, form highly charged supersaturated solutions, and retain their liquid state below ordinary atmospheric temperatures. When at a certain point they suddenly solidify. Other solutions merely deposit the excess of salt above the condition of supersaturation, leaving the mother-liquor saturated; the cupric sulphate is an example of this.

The memoir contains a number of details respecting the action of nuclei, whether derived from the air, from the flask, from the salt itself, from the filter, or the cotton-wool used in closing the vessels. If the solution touch the wool, crystallization immediately sets in; or if the upper part of a chemically clean tube be touched with a finger slightly greasy before filtering into it the hot solution, the latter will cool down to the temperature of the air without crystallizing, nor will there be any effect if the tube be inclined so as to touch the clean portions of the inner surface; but the moment the solution comes

upon the edge of the finger-mark, crystallization sets in, and the solution becomes solid. Solutions not filtered that begin to crystallize at above 100° in open vessels, or even in closed flasks, may by filtration be freed from nuclei, and so cooled down in the latter to low temperatures without any separation of the salt.

4. *On the formation of a modified salt.*—The readiness with which sodic sulphate parts with its water of crystallization, and two or three other considerations, make it more than probable that a solution of sodic sulphate at high temperatures is really a solution of the anhydrous salt. But M. Löwel supposes that a supersaturated solution in cooling down below 60° assumes a new molecular constitution, viz. that of the more soluble 7-atom hydrate which it then holds in solution. The author gives an experiment to show that such cannot be the case, but that the solution continues to hold the anhydrous salt until a portion of it actually separates. If a boiling solution of two parts salt to one part water be filtered into vessels made chemically clean by being washed out with spirits of wine instead of sulphuric acid, and if these vessels, when cold, be placed in water at 32° , or from that to 40° , a few octahedral crystals of the anhydrous salt will be thrown down. The temperature will slightly rise; and if the tube be now set aside in a moderately warm air, the anhydrous salt will enter into solution, forming a dense lower substratum, from which the 7-atom hydrate will be produced in small quantity, there not being sufficient water present to form the ordinary 10-atom salt. The rest of the solution is still supersaturated; and if the plug be removed from the vessel, crystallization will set in from the surface and proceed rapidly downwards, carrying down enough water to convert the whole solution, as well as the 7-atom, into the 10-atom hydrate.

This process may be conveniently watched in the case of the zinc sulphate. When a saturated solution of this salt cools down from the boiling-point to about 70° , the monohydrated salt is thrown down in quantity, and, as the solution cools, a portion of this dissolves and a crop of acicular crystals is produced which readily melt down at about 100° . On removing the cotton-wool from the tube, crystallization sets in from the surface, and the ordinary 6-atom hydrate is produced.

The author examines M. Löwel's experiments on solutions of the sodic carbonate in which two modified hydrates are pointed out, viz. the $7\text{H}_2\text{O}$ *a* and the $7\text{H}_2\text{O}$ *b*, which differ in solubility from each other and from the 10-atom salt; but as M. Löwel attaches great importance to the peculiar catalytic properties of the sides of his vessels in determining the formation of these salts, the author cannot help thinking that M. Löwel's results were due to portions of the sides of his vessel, not chemically clean, acting as nuclei. In chemically clean vessels M. Löwel's results have not been reproduced; for on reducing the temperature to a certain point depending on the strength of the solution, the whole became suddenly solid, with a rise in temperature of 35° or 40° . M. Löwel also points out two modifications produced from supersaturated solutions of the magnesia sulphate. The author has placed boiling saturated solutions, when cold, in

freezing-mixtures at 10° without producing any separation of the salt.

The ammonia phosphate throws down from its supersaturated solution an anhydrous powder, which, again entering into solution, forms a dense lower stratum in which a modified transparent crystallized salt is formed in small quantity.

The strontic nitrate also deposits an anhydrous salt in cooling down to about 62° ; but as this salt is not soluble in the solution, the modified salt is not formed.

Some solutions on being cooled down in freezing-mixtures suddenly become solid; others freeze and sometimes thaw again without any separation of the salt, as in the case of the cupric sulphate; but if a boiling saturated solution of this salt be prepared with strict attention to chemical purity, it may be cooled down to near 0° F. without any separation of the salt.

5. *Anhydrous Salts.*—The method adopted to ascertain whether an anhydrous salt forms a supersaturated solution was to make a solution of known strength, as indicated by some good Table of solubilities, raise it to the boiling-point, and then note whether salt began to be thrown down when the solution cooled down to the temperature indicated by the Table. For example, according to Poggiale's Table, 100 parts of water at 158° will dissolve 129.6 of sodic nitrate. This is the same thing as 622.22 grains of the salt in 1 ounce water. Such a solution on cooling down from the boiling-point began to deposit salt at 160° .

In like manner, according to Gay-Lussac's Table, 100 parts of water at 150° F. contain 125 of potassic nitrate. A solution of 125 parts salt to 100 of water began to deposit salt at about 149° . The deposit first began to be made on the side nearest the window, or the coldest side, when the flask was suspended in air; but if the flask were placed on metal, or any other good conductor, a ring of salt was first formed at the bottom, some 6° or 8° earlier than if the flask stood on a block of wood.

It has been frequently stated that the potassic bichromate forms a supersaturated solution. According to Kremer, 200 of water at 140° F. dissolve 100 parts of the salt. Such a solution, on cooling from the boiling-point, began to throw down crystalline flakes at 138° . The remarkable deepening in colour of this solution under the influence of heat is pointed out.

Sal-ammoniac, potassic chlorate, and some other salts were also examined, the conclusion being that anhydrous salts do not form supersaturated solutions.

6. *Conclusion and Summary.*—The author refers to the prevailing theory that supersaturation exists in appearance only and not in fact, since it is supposed to be the modified and more soluble salt that is in solution. If this were true, it ought to apply to all cases of supersaturation; and it has only been claimed in the case of a very few salts, and in them much importance has been attached to the active or the inactive condition of the sides of the vessels containing the solutions.

The author, while admitting, in the case of a very few solutions,

that a modified salt may be deposited, denies that it is due to any molecular change that takes place in the solution, either from reduction of temperature or any catalytic property of the sides of the vessel. His theory is that, when these modified salts are formed, it is the anhydrous salt that is held in solution, a portion of which is thrown down as the temperature falls; and this anhydrous deposit, entering again into solution, forms a dense substratum containing less water than the upper portions, so that when the modified salt forms in it, it is out of the reach of sufficient water to form the normal salt. When, on the contrary, under the influence of a nucleus, crystallization sets in from the surface, the normal salt is formed, and the crystals carry down sufficient water to convert the whole into the ordinary hydrated salt.

As to the action of nuclei or the sides of the vessel, when chemically clean the solution adheres to them as a whole, and there is no separation of the salt; when not chemically clean there is a stronger adhesion between the salt and the nucleus than between the salt and the solvent, and there is a separation of salt; and the action of separation once begun, may be rapidly propagated throughout the whole solution. Boiling saturated solutions may be cooled down in chemically clean vessels and kept for any length of time, not because they undergo any molecular change or hold a salt of greater solubility than the normal salt in solution, but they retain their fluid form simply from the absence of a nucleus.

The salts examined in this memoir are arranged into five groups according to their behaviour.

I. Salts of which the supersaturated solutions remain liquid at low temperatures.

Examples:—

Sodic sulphate.	Potash alum.
Sodic acetate.	Magnesia sulphate.
Sodic arseniate.	Baric acetate.
Sodic succinate.	Calcic chloride.
Sodic borate.	Cupric sulphate.
Sodio-potassic tartrate.	

II. Salts of which the supersaturated solutions suddenly solidify at low temperatures.

Examples:—

Sodic carbonate.	Sodic hyposulphite.
Sodic phosphate.	Strontic chloride.
Plumbic acetate.	

III. Salts of which the supersaturated solutions deposit their excess of salt at low temperatures or under the action of a nucleus, leaving the mother-liquor saturated.

Examples:—

Zinco-acetate.	Potassic arseniate.
Cupric sulphate.	Antimonio-potassic tartrate.
Baric chloride.	Citric acid.

IV. Salts of which the supersaturated solutions form modified salts of a lower degree of hydration.

Examples :—

Zinco-sulphate.
Sodic sulphate.

Magnesia sulphate.
Ammonia phosphate.

It will be seen that the sodic sulphate and the magnesia sulphate also occupy a place in Class I.

V. Anhydrous salts examined in this memoir that do not form supersaturated solutions :—

Potassic nitrate.
Potassic bichromate.
Sal-ammoniac.
Sodic nitrate.
Potassic chlorate.

Potassic ferrocyanide.
Baric nitrate.
Plumbic nitrate.
Ammonium nitrate.

“On the Tides of Bombay and Kurrachee.” By William Parkes, M. Inst. C.E.

The object of this paper is to exhibit the phenomena of diurnal inequality in the tides on the coasts of India, and describe the mode adopted by the author for obtaining formulæ based on astronomical elements for predicting them. It is accompanied by the following records of observations, given in a diagram form :—

Kurrachee,	1857–8,	December to March.
„	1865,	March to August.
„	1867,	The whole year.
Bombay	1867,	February to May.

The height and times predicted by the author for 1867, and published by the India Office, are given on the diagrams for that year, so that they may be compared with actual observation.

The continuous curves of the height of the water taken at Bombay, at every ten minutes for the four months above named, are also given.

By the rotation of the earth every meridian-line is brought twice a day under the influences which ultimately result in the well-known semidiurnal tidal movements—once when in the position nearest to the attracting body, and once when in that furthest from it. But the actual point in that meridian which is in the centre of those influences will be alternately north and south of the equator, to the extent of the declination of the attracting body. This alternation of the position of the centre of attraction from the northern to the southern hemisphere produces a diurnal tide, and that diurnal tide produces a diurnal inequality in the semidiurnal tide.

The character of the diurnal tide and the highly complex conditions under which its constantly varying solar and lunar component parts are combined are then traced. Being entirely dependent on the declinations of the sun and moon, the solar element vanishes twice a year, and the lunar element twice a month, each reappearing after the solar or lunar equinox, with its times of high and low water reversed.

The diurnal tide produces a diurnal inequality in height and time of high and low water, affecting simultaneously respectively high-water time and low-water height, and high-water height and low-

water time. In particular cases, the actual values of height and time of diurnal tide may be directly deduced from the values of diurnal inequality. From these it was found that diurnal tide follows the moon's movements at a much shorter interval than semidiurnal, the retardation of the former being from two to three hours only, while that of the latter is from thirty-four to thirty-six hours.

The mode adopted for identifying the varying values of diurnal inequality with their physical causes was then explained. A hypothetical series of diurnal tides, based on the varying values of the declination of the sun and moon, was calculated, the necessary local constants being deduced from the particular cases in which their values could be directly obtained. These hypothetical diurnal tides being combined with a series of semidiurnal tides deduced from the diagram of observations, the diurnal inequalities so obtained were compared with the actual diurnal inequalities. It was then found that a further element was wanting, which was approximately and provisionally obtained by the introduction of a second empirical diurnal lunar tide of twelve inches maximum half-range at Bombay, and six inches at Kurrachee. This tide was assumed, like the first and principal diurnal tide, to be dependent on the moon's declination, but to vanish at intervals of two or three days, before the moon crossed the equator. The author expresses an opinion that this empirical correction might probably be superseded by one more consistent with physical causes, if more extended and more correct observations were subjected to investigation.

Lastly, the comparison of calculated heights and times with the records of observations for four months at Bombay and eight months at Kurrachee were given. This showed that three calculated tides out of four were correct within three inches in height and fifteen minutes in time, the errors of the remainder ranging up to nine inches in height, and thirty minutes in time.

Since receiving the observations made at Bombay and Kurrachee in the year 1867, the author has subjected them to another process for obtaining the actual times and heights of diurnal tide, which has been more successful than that described in the paper.

The only data made use of were the diurnal inequalities in *height* at high and low water—the range of semidiurnal tide and the diurnal inequality in time, which were necessary to the previous process, being now altogether disregarded.

The diurnal inequalities in height were obtained by measuring the widths of the brown spaces where they were crossed by the vertical lines representing noon on successive days. The two daily values thus obtained are respectively the sine and cosine of an angle which represents the difference in time between semidiurnal and diurnal tide. Dividing the low-water by the high-water value gives the cotangent of that angle, and thence the angle itself. Thus the time of actual diurnal tide (first in relation to the time of semidiurnal low water, and then in relation to solar time) was obtained.

The actual range of diurnal tide was obtained by adding together the squares of the high-water and low-water values (sine and cosine), and taking the square root of the sum.

With these two series of results as ordinates, curves were drawn representing times and ranges of actual diurnal tide, which were thus presented in a convenient form for comparison with the diurnal tide which had been previously calculated.

The comparison confirmed the previous conclusion that the tide based on the simple declination theory was insufficient; and the empirical correction which had been adopted seemed to provide an approximation to the required addition to it, both in time and height. But it appeared that a better coincidence in time would have been obtained by assuming the diurnal tide at Kurrachee to be forty minutes earlier. This supposition was tested by treating the observations of 1865 in a similar manner, and also by recalculating a portion of the tides of 1867 with the earlier diurnal tide. In both cases the supposition was confirmed, a better agreement being obtained.

On treating the Bombay observations in the same manner, a fair general coincidence with the calculated diurnal tides was found to exist; but it was further found, on comparing together the Kurrachee and Bombay curves of actual diurnal tide (thus for the first time recorded for the same period), that the times were nearly identical at the two ports, and the range at Bombay about one-tenth greater than that at Kurrachee.

The tables for the four months over which the Bombay observations extend were recalculated with the diurnal tides which had been calculated for Kurrachee (but made forty minutes earlier, and increased in range by one-tenth); and the result was quite as good as that shown by the original tables. This fact would seem to point to the possibility that the diurnal tide is a vertical undulation, acting simultaneously, or nearly so, over a large area.

GEOLOGICAL SOCIETY.

[Continued from p. 158.]

May 6th, 1868.—Prof. A. C. Ramsay, LL.D., F.R.S., Vice-President, in the Chair.

The following communication was read:—

“On the Quaternary Gravels of England.” By Alfred Tylor, Esq., F.L.S., F.G.S.

Mr. Tylor first compared, by means of sections and models, the gravels of the Aire Valley at Bingley, of the Taff Vale between Quakers' Yard Junction and Aberdeen Junction, and of the Valley of the Rhonda near its junction with the Taff. He then described the cave-section of Bacon Hole, Gower, and the sections exposed at Crayford, Erith, and Salisbury, comparing the angles of deposition of gravel-beds concealing the escarpment of the chalk in these last three localities with the same conditions at Brighton and Sangatte.

By comparing the gravel-beds at different levels, and upon strata of different age and configuration, he showed in what respect they differ from each other. The bulk and height of the Quaternary deposits had strengthened the conviction which he expressed in his previous paper (on the Amiens gravel), that there was a long period,

reaching nearly to the Historical epoch, in which the rainfall was excessive, and which he termed the "Pluvial period."

These sections also led the author to the following conclusions:—

(1) That the *débris* was deposited by land-floods, and that the mode of deposition was quite distinct from that of moraines produced by the melting of ice. (2) That the character of the deposits in the valleys of the Aire, Taff, and Rhonda proves that they were formed under similar conditions. (3) That these gravel-beds point to a Pluvial period of great intensity and duration. (4) That the ice-action of which there is evidence was subordinate to the aqueous action. (5) That the fossiliferous Quaternary deposits have been best preserved where they have been formed in cavities lying between the edge of the bank of a river, estuary, or sea, and an escarpment running parallel with it at no great distance. (6) That the immediate source of the gravels was the high land adjoining the rivers, whence they had been washed down by rain, with the assistance of lateral streams, to the lower ground, where they had come into contact with larger quantities of running water, had been mixed with rolled materials, and spread in thick beds over the bottoms and slopes of valleys or the sides of escarpments. (7) That the surface of such a deposit rarely slopes at more than 2° or 4° , while the slope of the beds lower in the series near the escarpment averages 12° . The escarpment is usually concealed under a coating of gravel or loess.

Mr. P;RESTWICH dissented from the view of the author, that the valleys had been excavated to their present depth before the gravels were deposited; and, with reference to a former paper, explained that Mr. Tylor and himself had taken different points of observation near Montiers, and that his own views as to the separation which in some cases may be shown to exist between the high- and low-level gravels were correct.

Mr. EVANS also combated Mr. Tylor's views, and pointed out the difficulty of accounting for deposits of gravel such as are at present found in valleys already excavated to their present depth.

Mr. W. BOYD DAWKINS objected to calling in hypothetical causes to account for effects when existing causes are sufficient, and cited the sudden melting of snow as a sufficient cause, as had already been suggested by Mr. Prestwich.

Sir CHARLES LYELL supported the same view, and mentioned a case which had occurred at Salisbury some few years ago as an instance of the effects of such floods. He also cited the existence of flint implements in the gravels on either side of Southampton Water as evidence of the existence of man during a long period of excavation of valleys. He also mentioned the discovery by Dr. Harris of flint gravel identical with that of the present valleys beneath the basalt of Miocene date in Antrim.

Mr. SEARLES V. WOOD, Jun., insisted on the impossibility of even an enormously increased rainfall filling the valleys as suggested by Mr. Tylor, and pointed out the influence which such an accession of fresh water must have had on the animal life in the estuaries. He also mentioned tidal action as an excavating agent in valleys.

Prof. ANSTED showed, by calculation, that even a vast increase in the rainfall would not suffice to fill the valleys so as to deposit the gravels as at present found.

Mr. WHITAKER quoted the existence of distinct terraces of gravel one above the other in the Thames Valley as proving the gradual excavation of the valley.

Prof. MORRIS doubted as to the precise character and age of the deposits in the valleys in South Wales having been accurately ascertained.

Mr. TYLOR briefly replied.

Prof. RAMSAY made some concluding remarks, expressing his disagreement with the views of the author as to the enormous magnitude of the ancient rivers.

XXXI. *Intelligence and Miscellaneous Articles.*

ON THE PREPARATION OF CARBON TUBES, CRUCIBLES, ETC.

BY G. GORE*.

HAVING had occasion to require small rods and vessels of carbon, free (or nearly so) from silica, for experiments with hydrofluoric acid and with fused fluorides, I have been induced to devise the following method of obtaining them. Articles and vessels of the desired shape (but of sufficient dimensions to allow for shrinkage) were formed of different kinds of wood &c.; they were then allowed to dry in a warm place with occasional turning, then enclosed in a copper tube retort provided with two exit-tubes for the escape of gas, the tube placed horizontally between fire-bricks and heated with extreme slowness at first, and finally to bright redness, by means of a row of Bunsen's burners. It was necessary to continually turn the retort, and so to distribute the heat during the burning-process that none of the evolved tarry matter condensed upon the articles; otherwise it altered their form and dimensions in a remarkably curious and fantastic manner. The red heat was continued until gas ceased to be evolved. If the burning was too rapid, the articles fell to pieces or cracked very much.

The articles formed by this process consisted chiefly of rods, crucibles, boats, and tubes (the longest tubes made being $6\frac{1}{4}$ inches long, after burning); and the materials experimentally employed for their construction consisted of *lignum vitæ*, boxwood, beech, kingwood, ebony, ironwood, mahogany, zebra-wood, Memel oak, English oak, rosewood, "bastard rosewood," maple, lancewood, walnut wood, Norwegian pine, partridge-wood, "Braziletta," cocoa wood, vegetable ivory (or taqua-nut), coquilla-nut, and the hard shell of the cocoa-nut. The articles usually shrunk about one-fourth of their original dimensions during the process; but the amount of shrinkage varied with the different kinds of wood &c.; even with the greatest care many of them became twisted and shapeless during the process.

The best kinds of these various materials were found to be *lignum*

* Communicated by the Author.

vitæ, kingwood, ebony, and beech : rods made from most of these (if free from cracks) had a remarkably clear metallic ringing sound when struck ; those made from lignum vitæ were very hard and of exceedingly close texture ; those from beechwood best retained their form, but differed from all others in contracting unequally in the two directions across the grain of the wood, and, in consequence of this, round tubes and crucibles became oval during the burning-process. Rods made from lignum vitæ and the other denser kinds of material conducted electricity most admirably, and would probably serve well for carbon pencils for the electric arc ; but if they had been burned too rapidly their electric conducting-power was much less. Of the four materials above mentioned, lignum vitæ was the best.

The following were found to be the chief conditions of success in the process :—(1) vegetable materials of the hardest kind and closest texture ; (2) wood of the straightest grain, free from knots and splits ; (3) very slowly dried and heated ; (4) the heat very uniformly distributed ; (5) prolonged high temperature at the last ; and (6) gradual cooling.

Lignum vitæ evolved a very large amount of oily and tarry matter during the burning-process. The charcoal from it had a remarkably hard and shining surface, and two pieces of it rubbed together seemed like too smooth stones ; it was also very heavy, and its texture was so extremely close as to make it apparently quite impervious to liquids : even after immersion in the strongest hydrofluoric acid its surface had no acid taste ; it was feebly electro-positive to platinum in strong nitric acid, and therefore would not equal platinum, if substituted for it in a Grove's battery.

By the appearance of each kind of charcoal, the kind of wood from which it was formed could be distinctly determined, each wood retaining its usual superficial markings. Some kinds cracked transversely (for instance, boxwood and maple), others longitudinally (thus ironwood and partridge-wood) ; others twisted ; some kinds of charcoal were very hard (coquilla-nut, lignum vitæ, kingwood, and ebony) ; those of ebony and kingwood acquired a purple colour in places ; charcoal of beech and lancewood assumed a gentle curvature ; those of walnut and maple were easily broken across ; those of mahogany, pinewood, and walnut were soft ; those of English and Memel oaks were porous, &c.

ON MAGNETO-ELECTRICAL MACHINES.

BY MM. JAMIN AND ROGER.

In this research we have investigated, and believe we have succeeded in establishing, the laws of the production of electricity in magneto-electrical machines. These machines may be characterized by saying that they borrow from a motor, in the form of force, a given quantity of heat, and that they reproduce it, through the intervention of an electric current, in internal and external resistances. The question to be solved was to find the laws which regulate the quantity of heat borrowed on the one hand, and reproduced on the other.

Our machine consisted of six rotating plates, each provided with sixteen bobbins joined in tension and forming a total resistance R of twelve turns of the rheostat. These plates are joined in quantity, so as to form an electromotor of six independent machines sending their electricity into a common external circuit. The resistance of the whole is then equal to R divided by 6, or to two turns of the rheostat. In each series of experiments the velocity remained constant; it varied in the different series from 350 to 550 turns in a minute. The machines were driven by a Hugon's gas motor, the regularity of which has been proved to be perfect. By means of a brake on the principal shaft, the force, and therewith the heat imparted to the machine, could be regulated and measured. The heats regenerated in the external resistances were measured under the usual conditions.

All the experiments showed that the number C of thermal units thus regenerated in an external resistance increases as this resistance increases, diminishing then to zero when it becomes infinite. It attains a maximum for a value of x equal to R , or to twelve turns of the rheostat; it is exactly represented by the formula

$$C = \frac{x A^2}{(R + x)^2}.$$

But we know that in an external circuit x of a battery whose electromotive force is A and the internal resistance R , the heat regenerated is expressed by Joule's formula,

$$C = x i^2,$$

or by

$$C = \frac{x A^2}{(R + x)^2}.$$

The magneto-electrical machine behaves, then, as this battery would—with, however, an essential difference, which is that R does not represent its real internal resistance, which is $\frac{R}{6}$, but that of each of the plates or of each of the electromotors which concur in producing the total current. We may say, then, that Ohm's law applies to the magneto-electrical machine with an essential modification, by supposing that each of the various plates is independent, and that its currents accumulate on the external circuit.

There is an important difference between the battery and the machine. The quantity of heat C_1 furnished by the battery in a given time is proportional to the electromotive force and the weight of zinc dissolved—that is, to the intensity of the current; so that we have

$$C_1 = \frac{A}{R + x},$$

which shows that this heat varies as the ordinates of an equilateral hyperbola. But the magneto-electrical machine seems to be only a battery which borrows its heat from a motor instead of taking it from a chemical action, and we might be led to suppose that the quantity of heat furnished should vary according to the same law. This is not the case. The quantity is represented by the empirical relation

$C' = \beta + \frac{(x-\alpha)A^2}{(R+x)^2}$, in which α and β are constants. It is a minimum for $x=0$; that is, when the external circuit is zero; it gradually increases till it equals $\frac{A^2}{2(R+\alpha)}$ for $x=R+2\alpha$; it decreases to β when x tends towards infinity, which is the case when the circuit is open. It follows thence that, if the brake be not touched, the working of the machine is progressively retarded as the resistance increases up to a value of x equal to $R+2\alpha$, resuming then increasing velocities when the external circuit continues to increase.

These laws may be demonstrated in another way. Experiment first measures the quantity C or $\frac{x A^2}{(R+x)^2}$, then the increase of work $T-T'$ which the motor furnishes to the machine when, the circuit being first open, the resistance x is introduced. $T-T_1$, divided by the mechanical equivalent of heat E , represents the heat imparted by the motor or $y-\beta$; we have therefore

$$\frac{T-T_1}{E} = C - \frac{\alpha A^2}{(R+x)^2}.$$

By this formula E may be calculated.

Our experiments have given us more than fifty values for E , which agree virtually with those found by other methods.

The heat taken from the motor $\beta + \frac{(x-\alpha)A^2}{(R+x)^2}$ reproduces in the circuit a quantity $C = \frac{x A^2}{(R+x)^2}$. According to Joule's law, it should reproduce in the internal circuit a number of thermal units equal to $\frac{A^2 R}{6(R+x)^2}$, whence it follows that the difference between these quantities, that is,

$$C'' = \beta - \frac{\left(\alpha + \frac{R}{6}\right)A^2}{(R-x)^2},$$

represents the quantity of heat uselessly expended. Our experiments have shown that it is equal to two-thirds of that borrowed from the motor.

The discrepancies we have indicated between the laws of the magneto-electrical machine and those of the pile may be explained by an hypothesis which appears very probable. One portion of the heat C'' which is not utilized will be employed in overcoming passive resistances; it is constant; let us call it M . A second part will be employed in producing a reaction in the fixed magnets which cannot be experimentally valued, but which necessitates an absorption of heat; it is variable; let us call it C''' ; we have

$$C''' = \beta - M - \frac{\left(\alpha + \frac{R}{6}\right)A^2}{(R+x)^2}.$$

This reaction must then increase with the value of x , and attain its maximum when the circuit is open.

These laws being recognized and verified, we investigated the only application yet made of these machines, the electric light. Whenever a regulator is interposed in the path of the current, the velocity of the machine is diminished, as it would be by the interposition of a metal wire. The arc exerts a resistance x which can be determined by ascertaining how many turns of a rheostat must be introduced into the circuit to produce an equal retardation in the velocity of the machine. That being done, we have compared the heat disengaged by this arc with that reproduced in the resistance, and we have found that both are exactly equal. We have then been led to the conclusion that the arc only acts as a metal wire, either by the heat it regenerates, or by the diminution it produces in the intensity of the current.

This heat of the arc is very weak, scarcely equal to that produced by a gas jet burning a litre in a minute. To obtain this result, 100 litres of gas were consumed in Hugon's motor; and the heat reproduced does not exceed the hundredth of that consumed. But though small, as it is concentrated on a very narrow space on charcoal-points, it develops there an enormous temperature, and an intensity of light almost twice as much as would be obtained by directly burning the 100 litres of gas expended in its production, and four times as much, even, if the carbons are prepared by Carré's process.

There is thus an enormous loss of heat on the one hand, a remarkable gain of light on the other.

There is nothing paradoxical in this result. The magneto-electrical machine only utilizes a small portion of the heat absorbed; but it collects it disseminated over a large space, and concentrates it in a small volume; it takes it at a low temperature to produce an enormous heating of the carbons; it finds it as obscure heat to change it into light; it diminishes its quantity, and transforms its nature; it expends calorific radiations which are cheap, and makes them into luminous radiations which are dear; and it finally gives them at a cheaper rate than any other source of light.—*Comptes Rendus*, June 1, 1868.

ON THE EXPANSION AND COMPRESSION OF SATURATED VAPOURS.

BY M. CAZIN.

I beg to communicate the result of researches on saturated vapours on which I have been engaged for the last two years. The question was this—to observe under what circumstances a vapour is partially condensed when its volume is changed without either addition or loss of heat.

At the sitting of the Academy, January 25, 1866, I communicated a first series of experiments, on expansion alone. Since then I have constructed an apparatus by which the expansion or compression of vapours at high temperatures may be effected at pleasure.

A horizontal copper cylinder, closed by parallel glass plates and heated by the aid of an oil-bath, contains vapour in a state of satu-

ration ; its capacity is 6 litres, and its length 60 centims. A vertical cylinder of a litre capacity, communicating at its lower end with the preceding, is immersed in the same oil-bath ; it contains a piston of the same metal, which by being moved augments or diminishes the volume of the vapour. The rod of this piston passes through a stuffing-box at the top of the cylinder ; and the vapour can act upon the two faces of the piston. This vapour is supplied from a boiler containing the liquid, and which on the one hand communicates with the horizontal cylinder, and on the other with the upper part of the piston-cylinder.

When the oil-bath is at the desired temperature, the boiler is raised to the same point ; then the stopcocks are opened, so that the vapour is simultaneously admitted into the two cylinders. The two faces of the piston are then under the same pressure, and it can be moved without any great effort.

To effect a compression, for instance, communication is cut off between the boiler and the glass-plate cylinder ; a certain quantity of vapour is thus isolated in the apparatus. But the vapour is allowed to act on the upper face of the piston. The piston is then rapidly lowered, and the volume of isolated vapour is thus *momentarily* reduced, which is sufficient for observing the phenomena which take place in this vapour when it is compressed without either addition or subtraction of heat. The construction of the piston offers no difficulty, for it is not necessary that it perfectly retain the vapour.

The pressure of the vapour is given by a manometer fitted to the boiler and containing a column of liquid from the mercury to the boiler. By heating the tubes which conduct the vapour into the two cylinders, condensation is prevented and the vapour reaches the glass-plate cylinder dry and saturated.

The difficulties of construction have been very skilfully surmounted by M. Golaz ; and I have been able to observe the vapours of water, ether, chloroform, and benzine as high as ten atmospheres. The partial condensation of vapour produced by a sudden change of volume is manifested by a mist of very brief duration ; the action of the sides makes it at once disappear. When the mist is very dense, it is seen without difficulty when a mirror reflecting the light of the clouds is looked at through the glass plates ; but when very feeble, it has the appearance of a slight fume which undulates in the cylinder : the glass plates must in this case be very bright. There is one case in which this is impossible ; and that is, when the vapour is compressed. The action of the sides quickly brings the vapour to its initial temperature, and then a portion of this vapour is necessarily condensed. This condensation produces no mist ; but it dims the glasses, and thus observation is rendered difficult.

This inconvenience has been remedied by looking at the vapour through a telescope applied against the glass, which gives a sharp image of the central parts of the cylinder ; the glass plates are not seen, but the slightest mists which sudden change of volume in the vapour may produce are readily discriminated.

The following are some of the results of my experiments :—

Aqueous vapour is always rendered dull by expansion ; the mist

presents beautiful shades, according to its intensity. It is never rendered turbid by compression.

The vapour of ether has presented the reverse effect; it becomes turbid by compression and remains transparent by expansion.

These two results confirm those of M. Hirn, to whom the first observations on this subject are due.

The following are the new results which were the object of this research:—

The vapour of chloroform gave a mist when expanded up to 130° . At this temperature it was very feeble and was not easily obtained; the vapour remained transparent by compression. Above 136° , compression was accompanied by a very perceptible mist. The higher the temperature the more intense the mist, which was never observed with expansion. Experiments were made from 80° to 150° , and therefore up to nine atmospheres.

The vapour of benzole behaved like chloroform. There was mist by expansion alone up to 115° , and by compression alone above 130° . Observations between these temperatures were interrupted by an accident.

It has thus been experimentally proved that for these two liquids there is a temperature of *inversion*, below which the expansion of dry and saturated vapour, without either addition or subtraction of heat, is accompanied by a partial condensation, while above this temperature compression determines condensation.

There is thus verified a consequence of the formulæ of thermodynamics at which several authors have arrived by different methods.

The temperatures of inversion which I have calculated from these formulæ are 520° for water, 113° for ether, 123° for chloroform, 100° for benzole. The first of these numbers was found by Mr. Rankine in 1854; MM. Dupré and Combes have made analogous calculations for other liquids; and the differences of results are explicable by the choice of data, which are not accurately determined.

To give an idea of the degree of accuracy which my experiments present, I have calculated from thermodynamic formulæ the weights of chloroform vapour precipitated under the circumstances in which I worked:—

Original temperature.	Original volume.	Final volume.	Condensed vapour.	Thermometric effect.
	lit.	lit.		
100	5.844	6.862	0.174	-6.8
110	0.123	-7.3
120	0.070	-7.6
130	6.862	5.844	0.518	+8.5
140	0.644	+9.0
150	0.815	+9.6

The agreement between theory and experiment is as close as could be expected when we consider the want of purity in the liquids, and the uncertainty in regard to some of the data on which the calculations are based.—*Comptes Rendus*, June 8, 1868.

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[FOURTH SERIES.]

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XXXII. *On Some Effects of a Chemically Clean Surface.*
By CHARLES TOMLINSON, F.R.S.*

THE supersaturated solutions of a number of salts contained in chemically clean vessels can be kept during a long time without crystallizing, and even be reduced to temperatures much below the freezing-point of water, provided they are protected from the motes and dust of the air, and other chemically unclean bodies, which act as nuclei. For this purpose it is sufficient to close the mouths of the tubes and flasks with cotton-wool, which filters the air as it enters the vessel during the period of a rising barometer. When, however, the barometer falls, a portion of the air escapes from the tube, and carries with it some of the aqueous molecules of the solution; and the effect of this action often repeated is to lower the surface of the liquid and to deposit all along the width of the depression a crystalline crust of the salt. This crust is formed with singular facility in supersaturated solutions of magnesian sulphate; it may often be noticed in similar solutions of sodic sulphate, ammonia phosphate, and some others.

Many persons will think it a remarkable fact that this crystalline crust does not act as a nucleus to the solution itself when this is brought into contact with it. Nor does the solution dissolve it, even though the tube be kept inclined for days together so as to immerse a portion of the crust. It cannot do so, since its high state of saturation has more than satisfied its solvent powers. I say *more* than satisfied, because the water contains a much larger quantity of salt than it can take up at the temperature of the air.

* Communicated by the Author.

The reason why this crystalline crust does not act as a nucleus to the rest of the solution is, that, being chemically clean, there is perfect adhesion between it and the solution as a whole, and hence there can be no separation of the salt from the water of the solution.

An objection to this theory has been put :—In ordinary crystallization, as in nursing a crystal of alum, are we not dealing with chemically clean surfaces ?

My answer is a decided negative. The evaporating-dish that contains the solution is not chemically clean ; the solution itself, exposed as it is to the air, is not chemically clean ; nor is the hair by which the crystal is suspended ; nor is the crystal itself, for this is frequently taken out and exposed to the air and handled, and abnormal growths chipped off with the thumb-nail. Under such conditions of chemical impurity the crystal acts as a powerful nucleus ; and if the fresh crystalline deposits made upon it are chemically clean, they do not long remain so. A chemically clean octahedron of alum is transparent ; a nursed crystal is opaque, (as I believe) in consequence of the multitude of minute crystals formed upon the impurities of the surfaces.

The general opinion is that the most powerful nucleus for the crystallization of a saline solution is a crystal of the salt itself. It is nevertheless possible, with strict attention to chemical purity, to introduce a crystal into a highly supersaturated solution of the same salt, and yet the crystal shall not act as a nucleus, the solution remaining perfectly liquid as before.

For this purpose a solution of two parts by weight of magnesian sulphate and one of water was boiled and filtered into a flask made clean by means of strong sulphuric acid, and rinsing with water ; the solution was again boiled in the second flask ; and while steam was issuing from the neck, a short tube, suspended by a wire, full of crystals of magnesian sulphate (all previously dipped in sulphuric acid and rinsed with water), was held in the neck, and secured there by putting in the plug of cotton-wool at the same time that the spirit-lamp was removed. The flask was left to cool during about fifteen hours, and then, without disturbing the cotton-wool, the tube with the crystals was lowered into the solution : there was no crystallization ; and during forty hours, though the flask was shaken several times, the solution remained unchanged.

A similar solution was filtered into clean tubes, which were closed with cotton-wool, and when nearly cold were put into strong sulphuric acid, covered with a receiver, and the air exhausted. In the course of twenty minutes a crystalline crust formed on the surface ; and this, during the shaking of the pump, fell to the bottom of the solution ; but it did not induce crystal-

lization. After some hours the tubes were taken out, the cotton-wool removed, when the solution in one tube immediately became solid, and that in the other tube did the same the moment it was touched with a bit of wire.

I sent in a note of these experiments to the Royal Society by way of addendum to a paper "On Supersaturated Saline Solutions" which I had the honour of submitting to that body on the 28th of May last*. I also brought the substance of that note before the Chemical Section of the British Association at Norwich on the 20th ult. The subject excited some interest, and several gentlemen who spoke on the occasion called for a definition of a chemically clean, in contradistinction to a dirty surface, one gentleman quoting the adage that dirt is something in the wrong place.

As a suggestion from any one of the distinguished observers who spoke on this occasion must be highly prized by me, and as all of them seemed to think I had not sufficiently defined the basis of my theory, I adopt the course recommended by no less a man than Descartes—that is, not to answer an objection at the time it is made, but to carry it home, consider it well, and then reply to it in writing. I therefore suspend for a short time the further prosecution of this subject in order to define more precisely what I mean by chemical purity, and to illustrate that definition by briefly recapitulating the work that has been done by means of chemically clean surfaces. The work that remains to be done seems to me to be as important as any that has yet been accomplished. There are several sets of phenomena at present wrapped up in vague molecular theories, which I am presumptuous enough to think admit of simple explanation under the theory I am advocating.

In the first place, what is a chemically clean surface?

A chemically clean surface is one that has on it no film or coating of any substance whatsoever foreign to its own composition. As oxidation by the air, organic matter, and floating motes are the most usual forms of films, we might say loosely, that any substance which has been exposed for some time to the air is chemically unclean; but speaking strictly, a film of any foreign matter will render a surface unclean for some conditions or other in the experiments in hand.

A chemically unclean surface, then, may be generally defined as anything that is exposed to the products of respiration or of combustion, or to the touch, or to the motes and dust of the air, and so becomes covered with a film more or less organic. So

* Proceedings of the Royal Society, vol. xvi. p. 403. See also 'Chemical News' for July 3rd, 1868, and the Philosophical Magazine for September 1868.

also any vessel or surface wiped with a cloth that has been exposed to the air is chemically unclean. In some cases bodies that have been exposed to the oxidizing influences of the air become chemically unclean.

A good test of the chemical purity of the surface of water is afforded by camphor. If a few fragments be scraped from a fresh cut surface with the point of a knife, and be allowed to fall upon water, they rotate with amazing velocity, and sweep over the surface, if the water be chemically clean; but if not, the fragments lie on the surface perfectly motionless.

The morning of September 1st was sunny and bright and the air dry, conditions highly favourable to the camphor-motions, which depend as much on evaporation as on solution. On this occasion I washed four shallow glasses, A, B, C, D, each $3\frac{1}{2}$ inches in diameter, with strong sulphuric acid, rinsed them out with water, and then filled them with water from the cistern-tap. Camphor was very active on all four surfaces. I now put my finger into A, my tongue into B. Fresh fragments were inert upon A, but as active as before upon B—showing that the finger was chemically unclean, and that the tongue was clean, or rather that, instead of depositing a film on the water, it absorbed water and any possible film with it. The water was emptied from C, and the glass filled up from a so-called clean jug from the kitchen filled from the same tap that had been used for the glasses; but the camphor fragments on C were motionless, showing that the jug had imparted a film to the water in C which prevented adhesion. The water from D was also thrown out and the glass rubbed and polished with a dry cloth. On again filling up the glass from the tap, the camphor fragments were motionless on the surface of the water, showing that the glass-cloth, commonly called clean, had imparted a film to the water.

It will be seen from these experiments how easily a chemically clean glass or a water-surface may become contaminated and rendered chemically unclean. In some cases mere exposure to the air during a few minutes is sufficient to convert a clean into an unclean surface, and to arrest processes which appear to be going on with great vigour. In other cases these very processes provide for and maintain the conditions of chemical purity, as, for example, in the camphor-wheels and currents which I have elsewhere described*. A stick of camphor, held by forceps, is lowered so as just to dip below the surface of water which has been very lightly dusted with lycopodium powder. As soon as the camphor touches the surface, a clear

* 'Experimental Essays,' published in Weale's Series, p. 38, figs. 9, 10, and 11.

space of about two inches in diameter is formed as by a flash, arising from the production of a film of camphor, which, being instantly disposed of by solution and evaporation, makes way for another film, which disappears in like manner; and by this very process the surface is kept clean for further action. The solution travels on in lines corresponding with the faces of the stick of camphor, and as the lines reach the side of the glass they divide right and left, and are reflected inwards in curves, the effect of which is to form the lycopodium into pairs of revolving wheels, the two members of each pair revolving in opposite directions. I have had this effect in active operation during fifty hours, merely taking the precaution to lower the camphor as it became cut through at the surface of the water.

Another remarkable continuous action is maintained by camphor in what I have named "camphor pulsations"*. If a stick of camphor be lowered into shallow water, contained in a flat glass dish 6 or 7 inches in diameter, so that the camphor may touch the bottom of the vessel, the water will be thrown into rapid pulsations. On a fine bright day I have counted as many as 240 per minute; but the number rapidly declines to about 80, and the pulsations may continue at that number during some hours. So regular is the beating, that on one occasion a medical friend who came in while the experiment was going on instinctively took out his watch and counted the beats, which he said were exactly like those of the pulse. The effect is, indeed, merely a case of capillarity: the water rises some way up the camphor and detaches a film, which spreads by the adhesion of the water-surface; while spreading, the water sinks about the camphor-stick; but as soon as the film is disposed of by solution and evaporation the water rises again, detaches another film, which is disposed of as before; and thus the surface is kept clean for continuing the action. As the water becomes more and more impregnated with the camphor, the action slackens; and as successive films are detached all round the camphor-stick both above and below the level of the water, the action becomes more and more striking.

Of course in all these experiments, if the finger or other chemically unclean body be made to touch the water, the actions are brought to an end.

But it has been said, during the last century and longer, that if, while the camphor is rotating, the surface of the water be touched with an oil or anything oily or greasy, the camphor motions are instantly and permanently arrested. This statement requires considerable limitations. I have already shown† that

* *Chemical News* for 1863, vol. viii. p. 37.

† *Phil. Mag.* for September 1863.

if turpentine and other essential oils be newly distilled, a film of any one of them on the surface of water does not arrest the motions of the camphor; the fragments skate through the film and cut it up in all directions. Turpentine and similar oils by exposure or long keeping become oxidized; and such products of oxidation, being neither soluble nor volatile, form over the surface of the water a delicate but permanent network which prevents the adhesion of the camphor fragments.

Creosote behaves in many respects like camphor on the surface of water. I described its motions some years ago*; but in order to observe them the surface of the water must be chemically clean. If not, there is no adhesion, and the drop of creosote delivered to the surface from the end of a glass rod forms an inactive double-convex lens with well-defined edges. I have more recently shown† how strong is the adhesive force of creosote for the surface of clean water, as measured by its power of displacing the films of various oils, fixed as well as volatile, and of resuming its activity when it has been temporarily invaded by an essential-oil film. For example, a drop of castor-oil on water forms a beautiful cohesion-figure; but if a drop of creosote be placed by the side of it, the active creosote figure shuts up the oil figure into a lens and chases it about. When the creosote has disappeared, the oil flattens out into a large film covering nearly the whole surface. A second drop of creosote will plough through it, turning over and thickening the edges of the severed portions. The creosote will in like manner repel and cut up films of whale-oil, fish-oil, nut-oil, and several other fatty oils, showing how superior is its adhesion to the water as compared with theirs. The effects of many of the essential oils also afford some remarkable illustrations of adhesion. While the creosote figure is in rapid vibration, sailing about over the surface of the water, if this be touched with a drop of turpentine-oil, oil of cajeput, &c., the drop will flash into a film and cover the surface, suddenly arresting the motions of the creosote. But as the oil-film becomes thinner by evaporation, the creosote starts into life and repels or cuts through what may be left of the oil-film. When the volatile oil has disappeared, a second drop will form a film that will again arrest the creosote; but as the film gets thinner, the creosote begins to show twitches at the edge, and suddenly it becomes active and begins to sail about. This remarkable effect may be repeated a number of times on the same water-surface before the creosote has entirely disappeared.

In like manner a comparative Table of the adhesion of liquids to liquids may be formed by showing what films will displace

* Phil. Mag. for August 1861.

† Ibid. for June 1867.

other films. For example, oleine of fish-oil will displace castor-oil, pale seal-oil will displace oil of nutmegs, and so on.

In all these cases, although dealing with essential and fatty oils, strict attention must be paid to chemical purity. If the vessel or the water-surface be not chemically clean, the oils will form lenses and not films. But if clean, the oils, in being shut up or in moving about, leave no trails on the surface, and the creosote is as free and active in its motions as if no fatty oil were present. It is, in fact, a case of adhesion, in which the attraction between water and creosote is superior to that between water and the oils: even where an essential oil like turpentine arrests the motions of the creosote for a time, it does not displace the creosote; for this retains its hold on the water, and, when the adhesion of the oil-film has become weakened by evaporation &c., the creosote reassumes its rule over the whole surface.

There are, however, some liquids which carry with them certain purifying influences of their own, and impart them to the water and the vessel. Such are ether, absolute alcohol, wood-spirit, &c., which most readily form cohesion-figures, as noticed in my first paper on this subject*. There is also a phenomenon that may be noticed every time a glass of wine is drunk—known, I believe, as the “weeping of the wine,” or “tears in the cup,” and may possibly explain a passage in the Book of Proverbs (xxiii. 31) as to when wine “moveth itself aright” in the cup. When a glass is filled with wine (the stronger the better), the alcohol and vinic ether make the glass chemically clean; a sip is taken and the glass one third or one half emptied. The part of the glass between the original level and the reduced level is completely wetted by the wine, and there is a strong capillary action between the reduced level and the plate of liquid left on the glass. But the supply of liquid above the surface of the wine in the glass is kept up by a twofold action. In the spaces between the tears will be seen an ascending wavy current of liquid which rises (1) by the adhesive action of capillarity, (2) by the formation of a back current in consequence of the downward flow of the tears, just as a backwater is formed at the place where two currents of a river meet; and this action, in the case of a glass of wine, will be rendered more apparent, in the same way as we see it in a river, if there are any specks or floating particles moving on the surface to show its direction.

The motions of eugenic acid on the surface of water† form a kindred phenomenon on a horizontal plane, to the tears in the wine-glass on a vertical one. But in some cases tears are due

* Phil. Mag. for October 1861.

† See Phil. Mag. for July 1864 (Suppl.). This experiment also requires a clean water-surface.

to the evaporation and condensation of the liquid in the glass. This effect may be well shown by putting a little pyroligneous ether or wood-spirit into a deep glass, covering it with a receiver and exhausting the air. Under these circumstances it readily boils, and the vapour condenses at various points of the surface so as to form tears. The same effect may be shown by filling a long tube with spirits of wine, and then nearly emptying it, so as to clean the surface. If the tube be now fixed vertically and the flame of a spirit-lamp be applied below for a short time, tears will form during the cooling at different heights above the liquid. If spirits of wine be poured into a glass and a portion be then turned out, tears will form, but not nearly so well as in the case of wine. A little ether added to the spirit improves the effect. The explanation in this case is the same as in that of wine. Ether alone, or bisulphide of carbon alone, will not form tears, as the plate of liquid instantly evaporates. Benzole does not act well; naphtha acts tolerably well.

I have noticed tears formed by a very strong solution of the sodic acetate (5 parts salt to 2 water), which was boiled at 234° F., and filtered into a chemically clean flask, in which it was boiled again. It was then plugged with cotton-wool and left to cool. At about 90° the flask was put into water at 40° , and then into water at 32° containing snow. The glass above the solution now began to weep, the ascending and descending currents being well marked on the chemically clean surface.

The care bestowed by me upon chemically clean surfaces in studying the phenomena of supersaturated solutions seems to have greatly simplified what was a very complicated subject. Take the case of supersaturated gaseous solutions, such as soda-water, Seltzer-water, champagne, &c.* If any one of these be poured into a chemically clean glass no bubbles of gas will form on the sides, because the adhesion between the sides and the solution is perfect, and the sides may be regarded as a continuation of the liquid itself. A clean glass rod placed in the liquid does nothing more than form new sides, as it were, to the vessel, and its effect is merely that of the sides. In such case the rod will form no bubbles around it, and hence it is "inactive," because its adhesion is perfect. If dirty, the surface of the liquid surrounding it will be free; that is, there will be little or no adhesion between the water of the solution and the dirty surface; but there will be an adhesion between the gas of the solution and the unclean surface, and hence there will be a liberation of gas. All bodies that have been exposed to the air or to the touch are chemically unclean: they are covered with an organic film, as already

* Phil. Mag. for August and September 1867.

noticed, and act as nuclei in liberating gas, because such film prevents the adhesion of water, but does not prevent the adhesion of gas.

A precisely similar explanation may be applied to supersaturated saline solutions. In chemically clean vessels many of them may be preserved unchanged during a long time, and their very existence as supersaturated solutions depends in many cases on the absence of nuclei. Previous observers remark how sensitive some of these solutions are to cold; solutions that could be kept during the summer months begin to deposit crystals when the first touch of winter's cold is felt. I have kept such solutions unchanged during winter, and have put them into freezing-mixtures at 20° , 10° , and even at 0° F., and have frozen some of them without any separation of the salt. Previous observers are constantly referring to the supposed molecular changes undergone by the solution, or by the sides of the vessel containing it, when many of the results thus obtained are traceable to unclean tubes and flasks and thermometers. It seems to me to be impossible to study fairly or with profit the phenomena of supersaturation without reference to chemically clean conditions; and I believe the time is not far distant when such conditions will be considered essential in various chemical and physico-chemical inquiries.

Take a case of clean as compared with unclean. Five parts of sodic acetate and one part of water were boiled, filtered, and boiled again at 240° F. in a flask containing a thermometer which passed through a plug of cotton with which the flask was closed when the lamp was removed. The solution was left in a cold room during twenty hours, and then placed in a freezing-mixture during some hours. The solution had been some time at 14° , when, the thermometer being gently lifted up without being taken from the flask, the solution ran off from the bulb like a thickish syrup. On touching the solution with a wire it immediately solidified, and the thermometer rose from 14° to 104° F.

Such a solution as is here described crystallizes at 150° in an open evaporating-dish; and yet by observing the conditions required for chemical purity it may be cooled down to 14° , and probably to 0° , without crystallizing. It may be cooled at least 140° below its point of crystallization and yet not crystallize, simply because there is no nucleus to start the action. In the open evaporating-dish, on the contrary, no sooner is the boiling solution in contact with the air than its surface becomes dotted here and there with some of the specks and filaments that are floating about. If these be watched, one or more of them will be seen to increase in size by the aggregation of saline particles

about it, and suddenly from this point fans of crystals begin to radiate. In the meantime the sides of the vessel are active in separating the saline molecules, and the more so in proportion to the absence of chemical purity.

If the thermometer be taken out of the flask that contains a supersaturated solution and held in the air for a moment, it will catch some of the aerial motes, so that, on returning it to the solution, crystallization immediately sets in.

Air is not a nucleus; for it may be drawn through or shaken up with a highly supersaturated solution without any separation of the salt. The "mysterious action of the air," that several observers refer to, resolves itself into this,—that it acts the part of a carrier of some unclean mote or speck of dust which is the real nucleus. It had been noticed that supersaturated solutions can be kept longer in the liquid state in narrow-necked vessels than in wide ones; and the reason is obvious: the floating nuclei have a less chance of entering a narrow than a wide aperture. But even in such a case the locality of the experiment is of importance. If a flask containing a supersaturated saline solution be opened in a room, the solution will most likely crystallize the moment it is opened; but if taken into a field in the country, it may be kept for hours uncovered without crystallizing. Even so sensitive a solution as that of sodic sulphate I have kept in an open flask in the garden of my house without its crystallizing.

It is to my present purpose to notice in this place a fact communicated to me by Mr. Wentworth Scott after the reading of my paper at the last Meeting of the British Association, namely, that, on crystallizing saline solutions on a large scale in chemical manufactories (chlorate of barium for example), the workmen sometimes stretch clean white strings across the large enamelled vessels into which the solution is to be poured; and they find, practically, that the strings act best as nuclei when they draw them through their hands, "which," as my informant remarked, "are not particularly clean."

I might here introduce a number of facts respecting the boiling of water in vessels of different material, and the action of solid fragments in preventing jumping ebullition; but the subject is so extensive that I have included all I have to say respecting it in a separate memoir. I may merely mention here what is, I believe, a matter of common experience, that in introducing fragments of platinum, glass, &c. into retorts to prevent bumping, the fragments soon cease to be of any use. The reason is that they become chemically clean, and the liquid adheres to them with as much force as to the sides of the retort.

Dr. Faraday's experiment of firing the mixed gases by means

of clean platinum-foil is one of a large class of facts which also require special treatment.

There is a curious example of the production of chemically clean lines on the surface of glass by the discharge of a Leyden jar; a square of window-glass, four inches to the side, that has been exposed to the air so as to contract the organic film so often referred to, or dipped into a strong aqueous solution of soap and rubbed dry with a cloth so as to leave an invisible film of soap on the surface, will answer the purpose. If such a pane be presented to the knob of a charged Leyden jar (not a large one) and the discharging rod be placed with one knob on the outer coating and the other on the glass pane, the discharge will be made first on the glass next the knob of the jar, and it will pass round the edge and so get to the discharging rod. On holding the glass up to the light no electrical effect will be seen; but on breathing upon the glass, a remarkable tree-like figure will be traced out by the breath condensing in liquid lines wherever the electricity has burnt away the film and left the glass chemically clean, whereas on other parts of the glass, not touched by the discharge and where the glass is not clean, the breath will be deposited in minute beads of dew*.

I have already pointed out in this Journal† that the texture of dew depends greatly on the condition of the surface which receives it as to chemical purity. If chemically clean bottles, containing water and air or some gas, be placed near the window, we do not get deposits of dew, but weeping tears trickling down over sheets of water and leaving no furrows. In the article in the English Cyclopædia just referred to, I wind up with the following remarks:—"It is curious to notice the various modes in which the breath condenses on the glass plates, according to the temperature of the air and the amount of humidity or dryness, the tension of the electricity, and the state of health of the experimentalist. During the warm humid weather which occurred on certain days at the end of May and the beginning of June 1859, it was almost impossible to produce these lightning-figures. Then, again, in breathing on the plates a quantity of organic matter is deposited, which varies with the kind of food, the time that it has been taken, and the state of health of the individual. The breath, moreover, is differently deposited when projected by different individuals. The state of the breath is known to have a considerable influence in photography, and it

* I have named these figures "Lightning-figures:" see *Edinb. New Phil. Journal* for October 1861 and January 1862. See also the article "Breath-figures" in the *English Cyclopædia*, Arts and Sciences division, vol. i. col. 313. See also letter to the 'Times' newspaper, September 10, 1866.

† *Phil. Mag.* for May 1863.

has been noticed in other arts. In an old French treatise on enamel-printing (*Dict. des Arts et Métiers*, Lyons, 1801) the artist is cautioned not to let any one approach his work who has been eating garlic or taking mercurial medicines."

Variations in the amount of adhesive force, and distinctions between chemically clean and unclean surfaces, are constantly being met with on the great scale of nature. Water does not adhere to an oily or greasy surface; but air does so abundantly; and it seems necessary that it should do so in order to carry on the fermenting processes by which offensive organic matter is got rid of. For an opposite reason, salt will adhere to fatty matter, and so prevent those chemical changes which produce fermentation. It is seldom that we get perfect adhesion between water and a natural organic solid; mere exposure to the air prevents it more or less from retaining a chemically clean surface, even if it had one on its first appearance, as in the case of the young leaf or blade of grass. If the surface were chemically clean, the rain and the dew would completely wet it, evaporation would be more difficult, and there would be danger, from frequent soaking, of organic injury and even decay. But, in consequence of imperfect adhesion, and the presence also of a downy, powdery, oily, varnished, or rough irregular structure in foliage and flowers, and in the natural coverings of animals, the moisture forms in globules, which refresh the plant &c. while they remain, but are quickly dispersed by the wind or the heat of the sun. Inorganic matter, on the contrary, is rendered more or less chemically clean by every shower, and is more or less completely wetted; so that the roots of plants, or those parts which require most moisture, obtain it by a simple variation in the adhesive force of the constituents of the soil for water as compared with the living structures that grow out of it. The roots also are more readily wetted than the foliage or parts exposed to the air.

It is highly probable that all healthy animal and vegetable secretions are chemically clean, as well as the surfaces that secrete them. I have found that camphor will spin upon two or three such fluids when perfectly fresh; but they almost immediately change by exposure to the air, and then the camphor fragments are inactive.

There are various modes of obtaining chemically clean surfaces in flasks and other apparatus. Washing out with strong sulphuric acid, or with a strong solution of caustic potash, or with spirits of wine, and then rinsing with water, is generally sufficient. Soda-water poured into a test-glass thus prepared will not part with a single bubble of gas to the sides. When flasks are cleaned for highly saturated solutions of salts which

are to be heated or boiled again, it is well to rinse out the flask in which the solution is to be kept fluid with the acid of the salt, such as a little strong acetic acid for an acetate, nitric acid for a nitrate, and so on. For ammoniacal salts, a little solution of ammonia should be used. In this way, not only is the surface cleaned in the best manner, but any loss of acid or of ammonia during the heating is compensated. The flame of a spirit-lamp may often be used in cleaning such surfaces as those of platinum wire and foil, or of a glass rod, &c.

Highgate, N.
September 3, 1868.

XXXIII. On *Rhodium*. By R. BUNSEN*.

[With a Plate.]

IN working platinum ores on a manufacturing-scale, three products are obtained, which are principally used for the preparation of the other metals occurring along with platinum. These are:—

(1) The residues which remain after extracting the ores with aqua regia; they are rich in osmium and iridium, and are therefore especially fitted for the preparation of these metals.

(2) Osmium-iridium, which is obtained from these first residues by lixiviation, and which serves best for the preparation of ruthenium.

(3) The residues of the mother-liquors, which have been reduced by iron from the aqua-regia solution from which the platinum has been removed; being especially rich in palladium and rhodium, they are most conveniently used for obtaining these metals.

The following investigation was made with a material of this kind, which was supplied to me with the utmost liberality from the Imperial Mint in Petersburg. A kilogramme was used for each operation. These Petersburg residues contain all the platinum-metals with the exception of osmium, and are especially interesting owing to the large quantity of rhodium they contain. Claus, who worked on this subject, has endeavoured, if not to eliminate, at any rate to diminish the great difficulties incident to their preparation. The method he adopted, however, is extremely tedious, and scarcely practicable without sacrificing a great part of the valuable platinum-metals which accompany rhodium. In separating rhodium from iridium he used Wollaston's original process, which is based on the solubility, in chloride of ammonium, of the ammonium and potassium double

* Translated from Liebig's *Annalen*, June 1868.

salts of sesquichloride of rhodium. The circumstance that, as I have found, the bichloride of potassium and iridium is dissolved to a considerable extent by a solution of sal-ammoniac, or chloride of potassium saturated with rhodium-salt, must excite a reasonable doubt whether the metal thus prepared, and which has hitherto been regarded as the purest rhodium, and to which Claus, like Berzelius, assigned the atomic weight 52, does not contain considerable quantities of iridium. Hence it seemed necessary to forsake the old way and attempt a more exact method, so as to settle the doubts which the various and frequently discordant statements about rhodium still leave.

1. *Separation of Platinum and Palladium.*

The separation from platinum and palladium of rhodium, iridium, and ruthenium, which are insoluble in aqua regia, by digestion with this mixture, does not succeed with the residues in question; for a considerable portion of the latter metals is present partly in a finely divided state, partly in the form of hydrated sesquioxides, and consequently is dissolved in large quantities with the former metals—apart from the circumstance that the residue is filtered with extreme difficulty. But it is easy to extract platinum and palladium almost completely from the other metals by mixing the original material with one-third to one-half its weight of sal-ammoniac and heating the mixture gently in a Hessian crucible until the sal-ammoniac is *completely* volatilized and only vapours of chloride of iron are seen; the residue is then heated in a large porcelain dish with two to three times its weight of crude commercial nitric acid, and evaporated to the consistence of syrup. By ignition with sal-ammoniac, the metals not belonging to the platinum group are partially converted into chlorides; iridium, rhodium, and ruthenium are rendered insoluble, and the gelatinous silica present in the original material transformed into a pulverulent condition, so that it can be readily filtered. The chlorides formed from the sal-ammoniac yield, when digested with nitric acid, just as much hydrochloric acid as is sufficient for dissolving the platinum as bichloride; at the same time the copper and iron present reduce the palladium dissolved in nitric acid to such an extent that it is contained in the solution, not as bichloride, but as monochloride, which is not precipitated by chloride of potassium. Hence the mass, after treatment with nitric acid, need only be digested with water, filtered, and saturated with chloride of potassium, to obtain at once the greater part of the platinum as very pure bright yellow potassiochloride of platinum. This is washed with chloride of potassium and then with spirit, and the latter must not be mixed with the solution. The platinum precipitate weighed

62 grammes. The filtrate is introduced into a large stoppered flask, which must not be more than half full; if chlorine is passed into the flask which is shaken from time to time until no more absorption of the gas in the space above the liquid ensues, all the palladium is separated as a vermilion-red precipitate consisting of bichloride of palladium and potassium contaminated by platinum and some rhodium and iridium. This weighed 157 grammes. The liquid from which these precipitates are obtained is evaporated with hydrochloric acid not quite to dryness. On the addition of as much water as was necessary, by rubbing with a pestle, just to dissolve the chloride of potassium and the other soluble salts, a dirty chamois-yellow precipitate was left; this was separated by filtration, boiled for a short time with soda-lye and a few drops of alcohol, then mixed with hydrochloric acid until the precipitate at first formed was redissolved. After saturating the filtered solution with chloride of potassium, 13·5 grammes of chemically pure bichloride of platinum and potassium were obtained. The mother-liquor from which all these metals were separated, when tested with zinc and hydrochloric acid, indicated the presence of copper, but no platinum-metals. The separation of the vermilion-red palladium precipitate was effected as follows:—It was dissolved in boiling water, by which part of the bichloride of palladium was reduced to protochloride with disengagement of chlorine. After evaporating the solution with 60 grammes of oxalic acid and redissolving it in solution of chloride of potassium, 42 grammes of bichloride of platinum and potassium remained, which were yellow, and almost completely free from foreign admixtures. The washing was effected as before. The brown filtered liquid, which was somewhat evaporated in the water-bath, deposited on cooling 19 grammes of large and very beautifully formed transparent pea-green crystals of protochloride of palladium and potassium, which contained an admixture of chloride of potassium, but were found, on testing, to be free from other platinum-metals.

The liquid poured off from these crystals, carefully neutralized with soda, gave a slight precipitate of sesquioxide of iron and oxide of copper, which was removed. On the addition of iodide of potassium in the cold, all the palladium was separated as iodide. As iodide of palladium is soluble in iodide of potassium, an excess of the precipitant must be carefully avoided. This is readily effected if from time to time a little drop is taken from the outer edge of the liquid by means of a capillary tube*: as long as the precipitation is not complete, the drops brought against a white ground appear brown; when the precipitation is just complete,

* By this little device almost every constituent of a liquid which gives an insoluble precipitate may be volumetrically determined in a short time.

they are colourless; the least excess of iodide of palladium makes them appear wine-red. 77 grammes of iodide of palladium were obtained.

A specimen of the precipitate left on ignition pure metallic palladium, which dissolved readily and completely in nitric acid. The whole of the iodine used in the precipitation may be readily obtained in the form of chemically pure hydriodic acid, and be used for fresh precipitations. For this purpose the iodide is introduced into a tubulated retort, the neck of which is drawn out and bent down at right angles, and is fitted into a flask close over a layer of water. Into the tubulure is fitted a glass tube bound round with asbestos and cemented by gypsum, through which a slow current of hydrogen passes from a Döbereiner's apparatus to the iodide placed on the bottom of the retort. The retort is heated in a magnesia-bath (to be afterwards described) so strongly that almost only hydriodic acid and very little free hydrogen emerges from the drawn-out end. As the metal thus reduced retains very small particles of subiodide of palladium with great persistence, which render it partially insoluble in nitric acid, it must be strongly heated once more in a current of hydrogen.

The mother-liquor of this vermilion precipitate, which has thus been worked up for platinum and palladium, may still contain rhodium and iridium. It is evaporatēd to dryness with a little iodide of potassium, whereby a mixture of iodide of rhodium and iodide of iridium is separated; this is dissolved in aqua regia, and is separated by bisulphite of soda, as will afterwards be described—or, still better, is mixed with the material to be afterwards worked by this process.

2. *Separation of Ruthenium, and parting of Iridium and Rhodium.*

The residue, after treating with nitric acid a kilogramme of material, weighed 0.4 kilogramme. The platinum-metals had to be extracted in a form suitable for further operations. Hess's method of extraction by fusion with zinc alone is very imperfect, and attended with considerable loss. The following method, which depends on the deportment of zinc and chloride of zinc, is, I think, preferable to all others previously used.

If a small piece of zinc be melted in a porcelain crucible over a lamp, we know that it becomes covered with a coating of oxide. If iridium or any of the platinum-metals be added, this coating of oxide prevents any contact of the metal with the fused zinc, even if the metal be dipped in; but if a few grains of sal-ammoniac be thrown on the fused zinc, ammonia, hydrogen, and chloride of zinc are formed, and the latter at once dissolves the layer of oxide with the formation of basic chloride. The zinc at the same time closely resembles pure mercury in its lustre and

mobility, and in the readiness with which the small globules coalesce to larger ones. When the chloride has taken up as much of the oxide as it can, the layer of oxide reappears; but by being again sprinkled with sal-ammoniac this disappears. The fused zinc strewed with sal-ammoniac has another property in common with mercury—that of instantaneously adhering to many other metals, and, in case they form alloys with zinc, of rapidly dissolving them. When the sal-ammoniac is strewed on the surface, the chloride of zinc bubbles up, in consequence of the ammonia and hydrogen liberated by the zinc. Several oxides and chlorides, especially those of the platinum-metals, on coming into this layer of chloride, which is kept in rapid agitation by reducing gases, are reduced and rapidly dissolved by the zinc. On this reaction a very simple process is based of quantitatively separating platinum-metals from all substances which are neither reduced nor dissolved by zinc. If, for instance, osmium-iridium is immersed in the layer of chloride of zinc, in a few minutes it is completely dissolved by the zinc regulus at a temperature not much above that of the fusing-point of zinc, especially if the porcelain crucible, which is being heated by a Bunsen's burner, is kept for a few minutes in rotation. The gangue mixed with the grains of osmium-iridium is left in the basic chloride of zinc. If the regulus, after it has just solidified, be lifted out of the still liquid layer of chloride of zinc, and if together with the contents of the crucible it be rinsed in a porcelain dish with acetic acid just sufficient to dissolve the basic chloride, the gangue can be separated by filtration and quantitatively determined. In this operation the crucible is not in the least attacked; but if the regulus be not removed immediately after solidifying, the crucible is generally broken in cooling, owing to the unequal contraction of the metal and of the porcelain. When the operation is properly conducted, there is no trace of metallic globules in the slag. In order that the experiment may be made at as low a temperature as possible below the boiling-point of chloride of zinc, the alloy of zinc formed must not be too difficultly fusible, which is attained by having from twenty to thirty parts of zinc to one of the platinum-metals to be expected.

For the extraction of platinum residues which have been previously exhausted with nitric acid this method is admirably suited. By one fusion for two or three hours all the platinum-metals were extracted to the last trace. The details of the process are as follows:—From 3 to 3·5 kilogrammes of commercial zinc waste are fused in a Hessian crucible holding 2 litres, sal-ammoniac being occasionally added; the residue, weighing 0·4 kilog., which has previously been gently ignited with sal-ammoniac, is projected into it, and the mixture kept for two to

three hours at a temperature not much above the melting-point of the alloy to be formed, while from time to time, as the mass becomes too viscous, some dry sal-ammoniac is thrown upon it. The contents of the cooled crucible consist of three layers: the higher one, which is readily removed by the blow of a hammer, contains no platinum-metals; the second one (which is inconsiderable in quantity) contains a few porous difficultly fusible crumbs of an alloy of zinc and platinum-metals imbedded in the slag of chloride of zinc; the bottom one consists of what is often a very beautifully crystallized regulus. The second layer is coarsely powdered, softened with water, and the metallic crumbs separated by levigation and united with the principal one. To free this as much as possible from all adherent and enclosed impurities, it is once more fused with 0.5 kilog. of zinc, sal-ammoniac being added, granulated in water, and dissolved in hot fuming hydrochloric acid, which takes place with violent disengagement of gas, and scarcely requires half an hour. The chloride of zinc is used for the next operation. The platinum-metals, which settle as a heavy powder like metallic gold, may be washed by decantation without loss in the course of a few minutes. They are not pure, but contain considerable quantities of lead, copper, and iron, with traces of zinc, arising from the zinc as well as the platinum-metals used. They cannot be separated by means of nitric acid or of aqua regia; for a considerable portion of the platinum-metals dissolves up, while another is converted into insoluble oxides which are filtered and washed with difficulty. But, curiously enough, the platinum-metals may be almost completely freed from those impurities by digestion with hydrochloric acid. Not merely iron and zinc, but also *lead and copper are thereby easily dissolved with disengagement of hydrogen*. This is due to the electric current which is set up from the positive metals of the precipitate to the negative platinum-metals through the hydrochloric acid, by which electrolytic hydrogen is liberated on the platinum-metals, while the positive metals combine with the liberated chlorine. This reaction may be easily recognized by adding a few drops of solution of iridium to hydrochloric acid in which is a strip of copper, and which is heated to 100° C. The disengagement of hydrogen and solution of the copper commence as soon as it begins to be covered with iridium. The solubility of many metals is increased under these circumstances to a remarkable extent, as indicated by the following experiments made with moderately concentrated hydrochloric acid. The second column gives the volume of hydrogen which the same surface of the metals of the first column disengages alone, in twenty minutes, at a temperature of 100° C. The third column gives the quantities of hydrogen which are

given off under the same circumstances after the addition of potassiochloride of iridium.

I.	II.	III.
Palladium	0.0	1.5
Copper	0.0	18.4
Lead	23.5	58.0
Nickel	70.8	242.4
Tin	80.0	920.0
Cadmium	9.1	1040.0

The metallic mass separated from the zinc regulus, and alternately digested with hydrochloric acid and water, forms a fine dusty black powder, and, together with some admixed grains of sand arising from the crucible, weighed 65 grms*. It has the property of softly exploding with the appearance of fire when gently heated. The ignited and cooled powder, when brought in contact with cold unexploded powder, at once explodes the latter. If the explosion is effected by heating in an hermetically closed tube, either exhausted or filled with carbonic acid, neither hydrogen, nor chlorine, nor nitrogen, nor oxygen, nor aqueous vapour is liberated. From the method of preparation, these are the only bodies which could be present absorbed or in combination; it must therefore be assumed that all, or part, of the metals separated from the zinc are in a peculiar allotropic condition, which by heating, or by contact with non-allotropic metals, is got rid of with development of heat. The black powder which exhibits these explosive properties consists essentially of rhodium and iridium, with more or less considerable quantities of the platinum-metals originally present, as well as traces of lead, copper, iron, and zinc.

It is most intimately mixed with three to four times its weight of finely powdered perfectly dehydrated chloride of barium, and heated in a current of chlorine at a temperature which effects no disengagement of the gas.

The apparatus used for this purpose was arranged as follows. In the sheet-copper capsules, *c, c*, Plate II., provided with slides, *d*, and of which five or six could be arranged in a series, flasks of from 160 to 250 cubic centims. capacity were placed half full of the above mixture, and surrounded on all sides by ignited magnesia. Each flask is provided with a double-bored caoutchouc stopper, *ee*, and with two glass tubes; one of these tubes, through which the gas enters, passes to the bottom of the flask, the other, by which the gas emerges to enter the next flask, terminates just below the cork. The glass tubes are connected by india-rubber tubes, and are provided with bulbs

* A kilogramme of residues from other parts of the Petersburg stocks gave more than 120 grammes of this powder.

for the reception of any moisture that may pass over. In the flask *a* chlorine is disengaged, and is dried by passing through sulphuric acid in *b*; after passing through the series of flasks, it terminates in the chlorine-condenser, *f*, which is filled with wood-charcoal and solid hydrate of lime. Under each copper capsule is a single burner with a good draught, or, what is better, a three-jet Bunsen's burner. When all atmospheric air is expelled, all the burners are lit, and a brisk, but not violent, disengagement of chlorine is kept up for about three hours. The chlorine is at first so completely absorbed, that no bubble of gas escapes from the tube leading into the condenser when it dips under water. The successive action in the individual flasks is manifested by a deposition of perchloride of iron in the neck, and may be considered finished when this sublimate has about the same amount and appearance in each. On taking the apparatus to pieces, there is found in the flasks (the necks are cleaned from perchloride by bibulous paper) a dark rusty-brown mass not much agglomerated, which dissolves on the addition of water with great disengagement of heat. In six of the flasks, which may be used six or eight times for the same purpose, 100 to 120 grms. of platinum-metals may be attacked at once. The residue which remained unattacked weighed 13.7 grms., and after reduction with hydrogen 11.4 grms. By means of zinc, 4.5 grms. platinum-metals was extracted, which were insoluble in nitric acid, and which contained all the ruthenium present in the original material. Hence, of 65 grms. of platinum-metals in three hours, and by means of four ordinary non-luminous three-jet burners, 57 grms., or 93 per cent., was attacked by a current of chlorine, in the production of which 415 grms. pyrolusite of 85 per cent. was dissolved in hydrochloric acid.

The mass attacked by chloride of barium and dissolved in water contains, besides rhodium and iridium, chiefly copper, lead, iron, zinc, and small quantities of the other platinum-metals. The liquid is boiled, and the baryta removed by means of sulphuric acid. If specimens are taken by means of a capillary tube from the edge of the heated liquid in the manner mentioned under palladium, in a few minutes, by alternate addition of sulphuric acid and chloride of barium, any excess, either of barium or of acid, may be removed to within a hundred-thousandth of the entire saline mass.

In order to obtain the platinum-metals free from any impurities present in the solution, they are precipitated by hydrogen. For this purpose about a litre of the liquid is brought into a flask of twice the capacity, boiled for a time, and, after removing the lamp, the aqueous vapour is displaced by a rapid current of hydrogen; for this purpose it enters from a Döbereiner's apparatus

through a tube just projecting below a perforated cork, and emerges through a delivery-tube which is just over the layer of liquid. When the air has been completely expelled the exit-tube is closed, and the flask filled with hydrogen is connected, by means of a piece of thick caoutchouc tubing, with the Döbereiner's apparatus for the whole duration of the reduction. The temperature of the flask is kept just below 100° C. by placing it in a large iron pot which is heated by a small flame day and night. A water-bath at a constant level is better. The reduction begins at once, and is completed in five or six days if the quantity of metal amounts to about 100 grms. The first deposit consists principally of platinum and palladium; in the succeeding one rhodium preponderates; and the last parts are particularly rich in iridium. It is best to stop the reduction as soon as the liquid has assumed a greenish-yellow colour. The last traces of iridium which it still contains are obtained by evaporating the contents of the flask, igniting them with carbonate of soda, and afterwards exhausting the fused mass with aqua regia, so as afterwards to add them to another operation with chloride of barium. The reduction may be still more accelerated, if the liberated hydrochloric acid is removed from time to time by evaporation, care being taken to displace each time the hydrogen by aqueous vapour or carbonic acid before allowing air to enter, so as to avoid dangerous explosions. The palladium-metals separated consist of lustrous spangles, foil, and dendrites. Platinum and palladium are extracted by aqua regia, and, after treatment with soda, separated by chloride of potassium and iodide of potassium in the manner indicated above. The mother-liquor still contains some rhodium and traces of iridium, which are precipitated as iodide by repeated boiling with solution of iodide of potassium. These are dissolved in aqua regia and added to the subsequent separation of these metals.

The metals thus treated with aqua regia, and partially oxidized, are reduced by hydrogen at a temperature just below redness, and again converted into chlorides by being treated with chloride of barium in a current of chlorine. If from the solution of these metals the last traces of platinum and palladium are also removed by boiling with soda, and precipitation with chloride and iodide of potassium, there only remains the separation of iridium and rhodium. For this purpose the reddish-brown liquid is evaporated with hydrochloric acid, filtered, mixed with a great excess of bisulphite of soda, and left to stand in the cold for several days. Rhodium slowly deposits as an amorphous lemon-yellow soda double sulphite, while the solution becomes yellow and finally decolorized. With this change of colour, the rhodium precipitate also becomes brighter. The precipitate, well washed

out with water, contains the rhodium almost entirely free from all impurities. If the solution be warmed on the water-bath in a loosely stoppered flask, a fresh precipitate is formed, which is yellowish white and consists mainly of the above-mentioned rhodium-salt mixed with a greater or less quantity of the corresponding iridium salt. If, after filtering off this precipitate, the filtrate be evaporated to a small volume, two precipitates are formed: one is flocculent and of a yellowish white, which contains almost exclusively iridium with small traces of rhodium; the other consists of heavy crystalline scales which speedily fall to the bottom. This latter may be readily separated by washing and decantation; it weighed 16 grms. Whether this salt, which in general behaves like an iridium compound, contains a new metal I shall afterwards attempt to decide. The precipitates produced by sulphite of soda in the cold and on warming weighed, excluding that 16 grms., altogether 99.5 grms. In the mother-liquor, after adding hydrochloric acid and partial evaporation with zinc, traces only of platinum-metals could be detected.

The complete separation of iridium from rhodium is readily effected by merely treating the yellow precipitates with concentrated sulphuric acid. The precipitate is added in small portions to concentrated sulphuric acid, heated in a platinum crucible until sulphurous acid is disengaged, and the crucible heated in a sand-bath until all free sulphuric acid is volatilized and bisulphate of soda has been formed. On boiling out the contents with water, iridium is dissolved out as a sulphate with a deep chrome-green colour, while sulphate of rhodium is left in combination with sulphate of soda as a flesh-coloured double salt. It rapidly settles as a heavy powder, which must be boiled out with water and aqua regia, and washed by decantation. When I come to the determination of the atomic weight of rhodium, I shall dwell more fully on the constitution of this remarkable double salt, which is insoluble in hydrochloric or nitric acid or in aqua regia, can be heated to 250° without any change, and on ignition decomposes into metallic rhodium and sulphate of soda. The total quantity of this double salt obtained from a kilogramme of material amounted to 33.2 grms.

The first precipitates obtained by sulphite of soda in the cold yielded the rhodium almost entirely pure when treated in this way; the subsequent precipitates are very rich in iridium, and yield rhodium which is far purer than that prepared by previous processes, but not quite free from iridium. Hence these products obtained by sulphuric acid, which are not yet quite pure, and are recognized by their brownish tint, are collected, the rhodium separated by ignition, and the metal obtained heated once more with chloride of barium in a current of chlorine to purify it a

second time by the method already described. The green solution free from rhodium, but containing all the iridium, is freed from sulphuric acid by heating it, first in a platinum dish over the open fire, and then at a gradually increasing temperature in a porcelain crucible in a sand-bath; the porcelain crucible with its contents is finally strongly heated in a charcoal fire, by which neutral sulphate of soda and sesquioxide of iridium are formed. The latter remains as a black powder, after boiling out the fused mass, and is easily washed by decantation. It weighed 9.1 grms.

According to these experiments, there were thus obtained from a kilogramme of residue:—

	grms.
Chloride of potassium and platinum . .	117.5
Iodide of palladium	77.0
Chloride of potassium and palladium . .	19.0
Sulphate of rhodic oxide and soda . . .	33.2
Sesquioxide of iridium	9.1
Iridium containing ruthenium	4.5

The whole of the mother-liquors and residues from these preparations gave, when tested by zinc, inconsiderable traces only of platinum-metals.

In a subsequent memoir I shall recur to the constitution of the new compounds on which the method described is based, as well as to the atomic weight and properties of the chemically pure metal and some of its compounds. I will merely remark that working up 5 or 6 kilogrammes of crude material requires very little time, especially if all the filtrations are effected by a method which I introduced some time ago into my laboratory, and which consists in filtering in ordinary glass funnels with ordinary filter-paper under the pressure of a column of water of 25 to 30 feet. This pressure is produced by means of a water air-pump, on Sprengel's principle, which works day and night with a jet of water scarcely as thick as a straw, when by simply opening a tap it is connected with the water supply of the laboratory. The length of time required for washing precipitates is diminished to $\frac{1}{4}$ or $\frac{1}{8}$, especially in quantitative work, and the quantity of wash-water required is not more than from $\frac{1}{10}$ to $\frac{1}{4}$; immediately after filtering, the paper can be removed (even from gelatinous precipitates) like the skin of a boiled potato, so clean and completely, that the same filter may be used a second time; and the precipitates are obtained in such a form that they may be directly ignited without any further drying.

A minute description of this method, which, as regards certainty, precision, and rapidity, brings analysis by weight very near volumetric methods, I must defer to another communication.

XXXIV. *Note on the Resolution of Algebraic Equations.*

By THOMAS P. KIRKMAN, M.A., F.R.S.*

THE function Σ_β of my communication in the preceding Number is needlessly complex. If H_1 be any asymmetric function of the $n+1$ variables x_0, x_1, \dots, x_n , which is invariable by the cyclical permutation of all those variables, and which consequently receives all its values, suppose Πn values, by the permutations of $x_1, x_2, \dots, x_{n-1}, x_n$, and if Γ_r denote the group of r cyclical permutations of the first r of $1, 2, 3, \dots, n$, the remaining $n-r$ being undisturbed, and if

$$\Gamma_n H_1 = H_1^i + H_2^i + H_3^i + \dots + H_n^i = J_i$$

be the n values of H_1^i made by the substitutions of Γ_n , then I say that

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta = \Gamma_n \Gamma_{n-1}^\beta \Gamma_{n-2}^\gamma \dots \Gamma_5^\lambda \Gamma_4^\mu \Gamma_3^\nu \Gamma_2^\xi J_i^a$$

is a rational and symmetrical function of x_1, x_2, \dots, x_n , whatever be the *positive* integers $\beta, \gamma, \delta, \dots, \nu, \xi, a, i$. Here, as before, the exponent over Γ_r is intended to be written over the sum of r values made by its substitutions. This theorem is sufficiently proved in my paper of last month, and can be made further evident thus.

If
$$P = G + G_1 + G_2 + \dots + G_m$$

be any group consisting of a group G of any order followed by $m-1$ derivates of G , and if θ be any substitution of P , the operation θP can change nothing in the right member except the order of the terms G, G_1, \dots, G_m . If (P) be any rational function invariable by the substitutions of the group P , we can write it

$$(P) = (G) + (G_1) + (G_2) + \dots + (G_m),$$

where (G_r) is the value of (G) which is formed on G_r . It follows that if $(P)_i$ denote the sum of the i th powers of the $m+1$ values (G_r) , we have

$$(P)_i = (G)^i + (G_1)^i + (G_2)^i + \dots + (G_m)^i,$$

a function invariable by the substitutions of the group P . For as the operation $\theta(P)$ can do nothing more than change the order of the $m+1$ functions (G_r) , and as the substitution which changes (G_n) into (G_m) will change

$(G_n)(G_n)$ into $(G_m)(G_m)$, $(G_n)(G_n)(G_n)$ into $(G_m)(G_m)(G_m)$, &c., the operation $\theta(P)_i$ can make no algebraic change in the right member of $(P)_i$; that is,

$$\{(G)^i + (G_1)^i + (G_2)^i + \dots + (G_m)^i\}^k = (P)_i^k,$$

* Communicated by the Author.

whatever be the integers i and k , is invariable by the substitutions of the group P .

Now the function

$$\Gamma_2^\xi J_i^a = (J_i^a + {}_2J_i^a)^\xi = K_a^\xi$$

is invariable by the substitutions of the group Γ_2 ; and as

$$P = \Gamma_3 \Gamma_2 = \Gamma_{2+1} \Gamma_{2+2} \Gamma_2 = G + G_1 + G_2,$$

the function

$$\Gamma_3^\nu \Gamma_2^\xi J_i^a = (K_a^\xi + {}_1K_a^\xi + {}_2K_a^\xi)^\nu = L_\xi^\nu$$

is invariable by the six substitutions of the group $\Gamma_3 \Gamma_2$, whatever be the integers ν , ξ , a , i . Again, writing $G = \Gamma_3 \Gamma_2$,

$$P = \Gamma_4 \Gamma_3 \Gamma_2 = G + G_1 + G_2 + G_3;$$

whence

$$\Gamma_4^\mu \Gamma_3^\nu \Gamma_2^\xi J_i^a = (L_\xi^\nu + {}_1L_\xi^\nu + {}_2L_\xi^\nu + {}_3L_\xi^\nu)^\mu = M_\nu^\mu$$

is a function invariable by the twenty-four substitutions of $P = \Gamma_4 \Gamma_3 \Gamma_2$. Precisely in the same way, writing $\Gamma_4 \Gamma_3 \Gamma_2 = G$, we prove that N_μ^λ , the λ th power of the sum of five values of M_ν^μ , is invariable by the $\Pi 5$ substitutions of $P = \Gamma_5 \Gamma_4 \Gamma_3 \Gamma_2$, and finally we see that

$$\Sigma_\beta = \Gamma_n Z_\gamma^\beta$$

is invariable by the Πn substitutions of

$$P = \Gamma_n \Gamma_{n-1} \Gamma_{n-2} \dots \Gamma_3 \Gamma_2.$$

Hence the sum of β th powers Σ_β is a rational and symmetrical function of the n variables $x_1, x_2, x_3, \dots, x_n$; the sum of γ th powers Z_γ is the issuing root $\{Z_\gamma\}$ of a given equation of the n th degree; the sum of δ th powers Y_δ is the issuing root $\{Y_\delta\}$ of a given equation of the degree $n-1, \dots$ the sum of ν th powers M_ν is the issuing root $\{M_\nu\}$ of a given quintic; the sum of ξ th powers L_ξ is the root $\{L_\xi\}$ of a given quartic obtained by using $\nu=1, \nu=2, \nu=3, \nu=4$; the sum of a th powers K_a is the issuing root $\{K_a\}$ of a given cubic, obtained by using $\xi=1, \xi=2, \xi=3$; the sum of i th powers J_i is the issuing root $\{J_i\}$ of a given quadratic, got by using $a=1$ and $a=2$; and from $\{J_i\}$, using n values of i , we obtain, by solving a given equation of the n th degree, the required expression

$$\{H_1\}, \{H_2\}, \dots, \{H_n\}$$

of H_1, H_2, \dots, H_n , as irrational functions symmetrical in the n variables $x_1, x_2, x_3, x_4, \dots, x_n$.

This restatement and proof of the theorem of my former paper is, I believe, all true, and in part, I hope, new. The application that I made of it to the solution of equations of the $(n+1)$ th

degree, if $n > 3$, is, alas! all new and not true. My friend Professor Harley has shown to me that the irrationals $\{H_1\} \dots \{H_n\}$ are not symmetrical functions of the $n+1$ variables $x_0, x_1, x_2, \dots, x_n$, although the values of H_1 are invariable by their cyclical permutation. I hope the scientific reader will pardon the nonsense of the latter part of my preceding communication, and I wish that he may live to confess, like myself, that his mathematical powers are the worse for wear.

P.S.—Let me venture one word more. All that is required for the solution of $\Theta=0$ of the $(n+1)$ th degree, is the transformation of Π_1, H_2, \dots, H_n in the expression for x_0 in my preceding paper, into symmetrical functions of the $n+1$ variables Σx . By writing Π_1^a instead of J_i^a in Σ_β first above written, we can obtain, by solution of a given quadratic, $\{H_1\}$ instead of $\{J_i\}$, and we can write this

$$\Pi_1 = \{ {}_0 \Pi_1 \},$$

showing that Π_1 is transformed into an irrational symmetric in the n variables $\Sigma x - x_0$. But the Π_n values of H_1 are obtainable by the permutations of any n of the $n+1$ variables. We have got $\{ {}_0 \Pi_1 \}$ by permuting all but x_0 , which is undisturbed in the above process. We can repeat the process, permuting the n , $\Sigma x - x_1$, x_1 being now undisturbed. The result will be

$$H_1 = \{ {}_1 \Pi_1 \},$$

an irrational symmetric in the n roots $\Sigma x - x_1$. In the same way we can transform H_1 into the irrational $\{ {}_2 \Pi_1 \}$, symmetric in the n roots $\Sigma x - x_2$. And thus we transform H_1 into $\{ {}_r \Pi_1 \}$, symmetric in the n roots $\Sigma x - x_r$, whatever r may be, obtaining the $n+1$ results

$$\Pi_1 = \{ {}_0 \Pi_1 \}, \Pi_1 = \{ {}_1 \Pi_1 \}, \Pi_1 = \{ {}_2 \Pi_1 \}, \dots \Pi_1 = \{ {}_n \Pi_1 \}.$$

From these it follows that

$$\Pi_1^{n+1} = \{ {}_0 \Pi_1 \} \{ {}_1 \Pi_1 \} \{ {}_2 \Pi_1 \} \{ {}_3 \Pi_1 \} \dots \{ {}_n \Pi_1 \} = [H_1],$$

which is symmetrical in all the $n+1$ roots Σx ; and

$$\Pi_1 = [H_1]^{\frac{1}{n+1}}$$

is the required transformation of the rational asymmetric Π_1 into an irrational symmetric in all the roots, and therefore a function of the coefficients of $\Theta=0$.

If the reader will assist me so to handle the ambiguities of the implicated surds that we may be quite sure of all this (and if we can do it with H_1 we can do it with all the n H 's in the expres-

sion for x_0), then, after enjoying his laugh at my battle with the demonstrated impossibilities, he may come to think that we have beaten them after all.

The n groups of Πn which we use for the irrationals $\{_1H_1\} \dots \{_nH_1\}$ are obtained from that which gives $\{_0H_1\}$ by the cyclical permutations of $x_0x_1\dots x_n$, and complete with it the whole group of $\Pi(n+1)$. The $n+1$ tactical values of $\{_0H_1\}$, being numerically equal, depend on a binomial $(n+1)$ tic.

XXXV. On the Capillary Constants of Solid Bodies.

By Professor G. QUINCKE*.

THE phenomena presented by liquids in contact with capillary surfaces may either be referred to so-called molecular forces (that is, to attractive forces which the smallest particles of the bodies exert on each other at infinitely small distances), or they may be deduced from a tension which the surface of a liquid exerts like a stretched membrane, and which has the same value over the entire surface if the liquid is bounded by the same body.

Though the first point of view may be better suited for the accurate mathematical treatment to which it has been subjected by Laplace† and Poisson‡—to whom Gauss may perhaps be added, for his treatment§ combines both points of view—yet the second has the advantage of introducing a definite physical idea instead of the unknown molecular forces or molecular functions. With its aid Young||, by developing Séguin's¶ theory, has found the three fundamental principles of capillarity. MM. Hagen** and Plateau†† have more recently repeatedly called attention to the tension of the surfaces of liquids; and the experiments of the latter‡‡, as well as those of Van der Mensbrugghe§§, on thin liquid membranes are especially suited for demonstrating this tension in the surface of liquids.

Young||| first showed that the attractive forces exerted between

* Translated from the *Berliner Berichte*, February 1868.

† *Mécanique Céleste*, vol. iv. p. 389 (1805).

‡ *Nouvelle Théorie de l'Action Capillaire*, 1831.

§ *Principia Generalia Theoriæ Figuræ Fluidarum in statu Æquilibrii*, 1830.

|| *Phil. Trans.* 1804. Lecture on Natural Philosophy, vol. ii. p. 649.

¶ *Comment. Soc. Gott.* vol. i. p. 301 (1751).

** *Abh. der Berl. Akad.* 1845 & 1846.

†† *Mém. de l'Acad. de Belgique*, vol. xxxiii. (1861). *Phil. Mag.* S. 4. vol. xxii. p. 286.

‡‡ *Ibid.* vols. xvi., xxiii., xxx., xxxi., xxxiii., xxxvi. (1842–66).

§§ *Bull. de l'Acad. Roy. de Belgique*, vol. xxii. No. 11 (1866), vol. xxxiii. No. 5 (1867). *Phil. Mag.* vol. xxxiii. p. 270, and vol. xxxiv. p. 192.

||| *Miscellaneous Works*, vol. i. pp. 455 *et seq.* & p. 463 (1816).

the particles of a liquid produce an increase in the density on the surface of a liquid, and at the same time a tension on the free surface where it is bounded by a vacuum. He has then extended these considerations to the other surfaces of a liquid, which are bounded by other liquids or solids, and has shown that here also, for all points of the same limiting surface, there must be a constant tension, which, however, is smaller than at the free surface of the liquid.

On account of his extraordinary conciseness he is frequently obscure; and he has changed in the course of years; for from time to time he assigns the difference in the deportment of various substances solely to their different density.

But, from whatever point of view we start, we are always led to the conclusion that *the phenomena of capillarity must occur not only with liquids but also with solids, provided they are not absolutely rigid but permit a displacement of their smallest particles.* The author was originally led to this principle by the observation that fused substances, such as cement or glass, which when soft could form a surface of equilibrium, exhibit an unusual stability; and it seems in fact that a series of surprising phenomena may be unconstrainedly explained by assuming a tension on the surface of solids, whether they have a free surface (that is, bounded by air), or a surface in common with some other solid or liquid.

If the free surface of a solid is the same as that of a liquid upon which the same external forces act, its free surface must have a definite tenacity, measured by the pull exerted upon the unit of surface.

It must therefore be possible to express the tenacity of round metal wires by two numbers, the first of which is proportional to the section, and the second to the periphery of the wire. As the author was about to make experiments as to the validity of this law, his attention was drawn to a memoir by Karmarsch, which appears but little known*. Karmarsch, by numerous experiments on the tenacity of metal wires, arrived at exactly the same law which the author, without knowing those experiments, had deduced by theoretical calculations.

Karmarsch expresses the tenacity of metal wires thus,

$$F = aD^2 + bD; \quad . \quad . \quad . \quad . \quad . \quad (I)$$

where D is the diameter of the wire, and a and b are constants. The validity of this expression has been demonstrated in wires of gold, steel, iron, German silver, silver, brass, copper, platinum, and zinc from the most various sources, and in both annealed and unannealed condition. The difference between the observed tenacities and those which can be calculated from the values

* *Mittheilungen des gew. Ver. für Hannover*, 1858, pp. 138–155.

found for a and b does not, with individual exceptions, exceed a few per cent., which may be considered surprising when the possible sources of error are taken into account, and we remember the difficulty of obtaining constant results for similar determinations in the case of liquids.

As the formula holds for annealed wires, the author does not think with Karmarsch that the metal is first condensed on the surface by the pressure in the draw-plate and its texture advantageously altered. It does not follow thence that the increase in the density of the metal necessitates an increase of those constants a and b . On the contrary, the theory says that this increase must be greater for b than for a , in accordance with experiment.

Karmarsch gives the thickness of wires in millimetres, the tenacity in pounds. Taking f as the tenacity in grammes for wires of the diameter r in millimetres, or

$$f = \alpha 2\pi r + \beta \pi r^2,$$

we have

$$\alpha = \frac{b}{\pi} 500, \quad \beta = \alpha \frac{4}{\pi} 500; \quad . \quad . \quad . \quad (2)$$

and the values of the constants α and β are then easily deduced from the values of a and b given by Karmarsch. The following Table gives some values of α , where α is the constant of capillarity of solid metals at ordinary temperature (15°), and measures the pull exerted upon 1 millim. of surface. The numbers may be readily compared with the ordinary capillarity-constants of liquids, which are usually given in milligrammes. (Vide *Fortschritte der Physik*, 1863, vol. xix. pp. 68-74.)

Capillarity-constants of solid Metals.

	Hard-drawn.	Annealed.
	gr.	gr.
Iron	5731	1592
Platinum	3025	2388
Copper	2388	
Silver	2388	478
Gold	1592	478
Zinc	557	
<hr/>		
Steel	6685	955
Argentan	6685	1114
Silver	5253	2547
Gold (14 carats)	3661	2228
Brass (wire)	2547	1751
Brass (string)	1751	637

The values thus found are very great if compared with the

constants for liquids like water (0.008 gr.) or mercury ($\alpha = 0.055$ gr.). Yet, as Laplace has shown, these constants must increase with the square of the density if the function of attraction remains the same; and it is even possible that they experience a sudden increase near the change of condition, just as the density or the electrical conductivity suddenly changes. Experiment has also shown that, as the temperature of water and other liquids sinks, the constant increases. The author has observed the same fact in the case of mercury, in opposition to the statement of M. Frankenheim*.

Near their melting-points the capillarity-constants have comparable values, as will presently be shown. Hence metals with a high melting-point, apart from other differences, must indicate a high value for ordinary temperatures, as is seen from the above Table.

As a layer of liquid near the surface acts like a stretched membrane, the surface must offer to external impressions a resistance which is greater the greater the capillarity-constant. Hence the metals must be ranged according to the value of α in the same order as that of their hardness. Karmarsch's experiments†, in fact, as well as those of Calvert and Johnson‡, on the hardness of metals, give values in accordance with this relation which agree as well as can be expected in such determinations.

It might be thought that wires of the same section with a larger surface must exhibit greater tenacity. The author has made experiments in this direction with round and flattened silver and copper wires, and he has found that the tenacity with a flattened wire is almost the same as in the case of a round wire of the same section and the same material. It must at the same time be remembered that in flattening, not merely the density but the continuity of the surface is altered, as microscopic inspection shows, and the wire acquires cracks like flattened dough. Moreover the surface of such a flattened wire is not a surface of equilibrium, and other forces (the difficult displaceability of the particles) prevent the formation of the surface of equilibrium towards which the capillary forces tend. In the different parts of the surface there will be different tension. As, moreover, the section of the flattened wire is very irregular, a more accurate calculation would only afford interest in case we could make experiments between drawn wires having an elliptical and those having a circular section.

Gold and silver leaf have not a continuous surface, but are per-

* Pogg. *Ann.* vol. lxxv. p. 26.

† *Mit. des gew. Vereins für Hannover*, 1858, p. 178.

‡ *Ibid.* p. 175, taken from the Memoirs of the Literary and Philosophical Society of Manchester.

forated by holes; otherwise the tenacity of a piece of gold leaf must be greater than experiment shows to be the case. If we take care that these holes are wanting, or are present in small quantity, the tenacity is very great. The author has deposited thin layers of platinum, silver, or gold upon plane glass surfaces, and is astounded at the tenacity possessed by layers of 0.01 millim. thickness. A suitably formed piece of steel planes regular shavings in such a layer. Copper deposited upon the silver layer gives, together with the silver, a very firm membrane. On tearing from the glass, the side turned towards the glass is convex, the other is concave. The author thinks it probable that it is chiefly the tension on the free metallic surface (which, according to theory, must be greater than at the limit of glass) that produces this curvature. Hence a metal with a high capillarity-constant, like platinum, does not adhere to glass surfaces when it is deposited upon them in the cold, but acquires cracks and peels off. It is only after the tension on the surface has been diminished by strong heating that the metal adheres more firmly. If by the pressure of polishing the density and therewith the capillary tension of the metallic surface are increased, metallic layers become detached from the glass, which would otherwise adhere firmly. Measurements of the tenacity of such thin layers were not made by the author, owing to the difficulty of fastening and loading them.

If thin metallic laminae like gold leaf be raised to a higher temperature, by which the mobility of the individual particles is increased, they curve in consequence of the superficial tension (just as liquid drops form from a thin layer of liquid), and the pores of the thin metallic laminae become larger. This explains the phenomena observed by Faraday in thin metallic laminae*.

The principle adduced above, that in the common surface of two solids there is a capillary tension, renders it possible to foresee the explanation of a few other known but hitherto unexplained phenomena.

Two plates adhere more firmly the thinner the layer of liquid between them. In like manner, two solids stick the more closely the thinner the layer of cement or glue between them.

The tenacity of damask steel or rolled iron depends on the circumstance that in the manufacture care is taken to produce a great capillary-surface—the junction of heterogeneous substances. Different kinds of iron are packed over each other and joined by rolling and welding; it is not improbable that they remain separated by an extremely thin layer of oxide or of slag. Heated afresh between each welding, the soft masses may arrange their

* Phil. Trans. 1847, p. 145. Faraday, 'Experimental Researches,' vol. iv. pp. 401 *et seq.*

common surface in such a manner that the capillary tension on them is constant and attains the maximum value which is possible for the common limiting surface of the two bodies.

If this arrangement be changed, by the force of crystallization for instance, the fibrous structure changes into a crystalline one, and the solidity must become much smaller.

In drawing wire, hollow cylinders of metal of differing density glide over each other. The heat developed by the drawing contributes to render the metal particles more readily displaced, so that on the common surface of two such hollow cylinders the capillary tension is a maximum. The tenacity of hard-drawn wires must be greater than that of annealed ones. In physical mixtures of heterogeneous substances (alloys, steel) wire-drawing must favour the formation of capillary surfaces more even than in the case of homogeneous metals; so that in these the decrease of the capillarity-constant is most marked. All this agrees with observation.

The great tenacity of bodies which are built up of thin systems of tubes, like bones, the tenacity of organic substances which consist of individual cells with large capillary surface, are explained by this capillary tension. If these solids have been originally formed from soft masses, on their solidifying there has been a constant tension in the entire capillary surface as great as was possible under the given circumstances. The changes which this tension of the surface subsequently undergoes in consequence of alterations in temperature or of the form of the surface, will in general be small in comparison with the whole value on solidification.

The alteration in the measures used for geodetic purposes, which have recently been demonstrated by M. Beyer*, may partially arise from the usual prismatic form, which agrees so little with a surface of equilibrium. In spheres, solid as well as hollow, there is little reason to apprehend changes of this kind.

The difficulty of assuming forces near the surface of solids which depend on the form of the surface, in opposition to traditional ideas, is not in fact so great as might at first sight appear. Displacements of the molecules of what are called solids have long been known. The plasticity of ice†, the phenomena of regelation‡, the experiments of M. Treseca on the efflux of solids§, the different deportment of the same substance according as it is in the crystalline or the colloid condition, indicate a more or less prominent community of properties in solids and liquids.

A rigidly theoretical treatment of the capillary phenomena of solids is rendered more difficult by the circumstance that the

* *Berliner Monatsberichte*, 1867, p. 1.

† Forbes, *Phil. Trans.* 1846, p. 143. Tyndall, *Phil. Trans.* 1857, p. 327.

‡ Faraday, *Phil. Mag.* vol. xvii. p. 162 (1859), vol. xxi. p. 146.

§ *Comptes Rendus*, vol. lix. p. 754 (1864); vol. lx. pp. 398 & 1226 (1865).

surfaces are not surfaces of equilibrium, as in the case of liquids, that therefore, as in these too, we are ignorant of the nature of the molecular forces and the distance at which they act.

For this reason the author has directed his attention for the present to actual liquids, and has endeavoured to determine the capillarity-constants of fused bodies, especially metals, of which hitherto little was known. His expectation, that the capillarity-constants of metals near their solidifying-point must have comparable values, he has found confirmed. From the nature of the case these determinations must be considered not more than approximations to the true value, and indicate greater discrepancies than is the case with the determinations of liquids at ordinary temperature, like water, mercury, alcohol, &c.

The noble metals, in the form of vertical wires, were melted in a flame at as low a temperature as possible, until the suspended drop fell. Neglecting the small quantity of fused metal adhering to the thin wire, the weight P of the drop is the greatest weight which the fused vertical mass can support, or

$$P = \alpha 2r\pi; \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where $2r$ is the diameter of the wire in millimetres, α the capillarity of the fused metal.

In the same manner were heated glass threads which had been drawn out of the same glass rod at the blowpipe-table.

The flame used was generally a small coal-gas one, of 10 millims. height and 3 millims. diameter. Only in the case of platinum was a current of oxygen passed into this flame from an ordinary blowpipe. Special experiments with gold wires showed that flames at high temperature give drops which are comparatively smaller only because the highest layer of the surface of the drop touches solid metal, and must therefore be near the fusing-point of the metal. In thick wires the influence of the high temperature of the flame must have least play, and hence these give in general the highest values of the constants α . In this arrangement of the experiment the author thought the highest values of α to be the most trustworthy.

The following are measurements on a series of platinum wires in confirmation of the relation expressed by equation (3):—

Diameter of the wire. $2r$. millim.	Weight of the drop. P . gr.	Capillarity- constant. α . mgr.
0·5675	0·2912	163
0·3689	0·2055	177·4
0·1921	0·0996	165·1
0·0993	0·0530	169·8
0·0767	0·0410	169·9
		169·04

The other metals dropped out of a glass funnel which terminated below in a vertical tube of the diameter $2r$ in the clear. The magnitude of the drop is then also determined by equation (3); yet it is far more difficult to obtain concordant results than by the above method, even if the velocity of efflux of the fused metal (which determines the size of the drop) is made as small as possible.

The capillarity-constants of fused metals α thus determined are collated in the following Table, and, as a comparison, the values found for mercury* and water† are added. The values of α are given in milligrammes. The constant a , by which Poisson designated the height to which a liquid rises against a vertical side that it moistens, may be calculated by the aid of the equation

$$\alpha = \frac{a^2}{2} \sigma,$$

in which σ is the specific gravity of the metal in question as compared with water.

	Capillarity- constant.	Fusing- point.
	α . mgr.	
Platinum	169.04	> 1600°
Gold	100.22	1200
Tin	59.85	230
Lead	(56.14)	330
Mercurey	55.21	— 40
Silver	40.94	1000
Glass	18.09	(1100?)
Water	8	0

XXXVI. *On certain Silicic Acids and the Anhydrous Mineral Silicates.* By J. EMERSON REYNOLDS, *Member of the Royal College of Physicians, Keeper of the Minerals and Analyst to the Royal Dublin Society*‡.

THE recent changes which have taken place in the modes of viewing chemical reactions and the constitution and metamorphoses of the products of our laboratories, have hitherto exercised but little influence upon the study of mineralogy. Efforts have been made, as will presently appear, to replace the old equivalent formulæ of minerals by the atomic notation now

* Pogg. *Ann.* vol. cv. p. 33 (1858).

† *Fortschritte der Physik*, vol. xxxi. (1867) p. 107.

‡ Communicated by the Author.

generally employed in expressing chemical changes. But little attention has hitherto been given to these attempts to bring mineral chemistry into accordance with other branches of the science. This indifference may have resulted, on the one hand, from a dislike to alter conventional modes of expression, and on the other from the consciousness that the new formulæ were less simple, though perhaps more accurate, than those written according to the old plan. Whatever the causes may have been, the fact is that few use the "new system" in writing the formulæ of minerals. That a change is most desirable I think few will deny; the question really is how such can be advantageously effected. The adaptation of the old formulæ of the mineral oxides, hydrates, sulphates, carbonates, phosphates, sulphides, arsenides, &c. to those of the new system is a very easy matter. But the alteration in the case of the silicates is very considerable; and since these bodies constitute nearly two-thirds of the solid crust of the globe, it is of great importance that any formulæ by which their composition may be represented should be capable of exhibiting the natural relations of individuals.

We have already had the advantage of ascertaining the views of Dr. Odling* on this subject, of Dr. Frankland†, Mr. Watts‡, M. Adolphe Wurtz§, M. Naquet||, and others; but it must be confessed that we have not yet arrived at a satisfactory mode of expressing the constitution of the silicates, notwithstanding the amount of ingenuity and chemical ability which has been brought to bear upon the question.

Professor Dana, with his usual care and the clear appreciation of scientific truth which marks his works, has entered largely into the subject of the unitary formulæ of minerals in the new edition of his 'Descriptive Mineralogy,' just issued from the press. In most cases he gives the formulæ of minerals on both systems; but in writing those of the silicates he departs to some extent from the ordinary rules even of the new system, and adopts the notation proposed by him in his recent papers on the subject¶. Whether the change is an advantageous one or the reverse remains to be decided; but since the new formulæ, as given by

* Philosophical Magazine, 1857.

† Lecture Notes, 1866, pp. 97 & 175.

‡ Watts's 'Dictionary of Chemistry,' 1863, especially vol. v. pp. 240-263.

§ Modern Chemical Philosophy. The valuable translation of this work by Mr. William Crookes, F.R.S. 1867, p. 157.

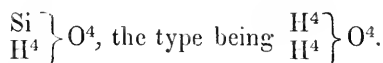
|| Principles of Chemistry: founded on modern theories, translated by W. Cortis: 1868, p. 135. In Naquet's work, only the hydric silicates are treated of; but these are regarded as types of the native metallic silicates; hence the views of this chemist on the constitution of most of the latter salts can be inferred from his statements in reference to the former bodies.

¶ American Journal of Science, vol. xlv. pp. 145, 252, 398.

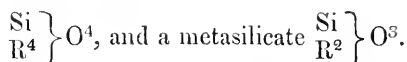
Dana, do not appear to contrast favourably as regards simplicity with the older ones, I venture to bring forward some views which I have long held on the symbolic notation for the anhydrous silicates and on some points in their constitution. These views were expressed in substance at one of the meetings of the Royal Geological Society of Ireland last session; and the present juncture seems a favourable one for discussing them and exhibiting some advantages derivable in practice from their application.

In the following observations it will be understood that the new atomic weights for the elements are those employed, unless otherwise specified.

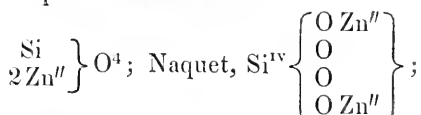
It is usual at the present time to represent the silicates as constructed on the type of water; thus the normal* hydrate of SiO^2 is



A normal silicate is therefore



This, as we all know, is the general plan adopted by modern chemists for expressing the composition of these salts, variously modified, however, by each author to suit his idea of the mode of representing the constitution of the molecule. Thus Odling or Wurtz would represent *Willemite* as



Frankland, $\text{Si ZnO}^{2''}$; and Dana, $\text{Si} \parallel \text{O}^4 \parallel \text{Zn}^2$.

On ordinary grounds there can be no objection to the use of the formulæ just given; but practically they all have the very serious defect of hiding the "oxygen ratio" of the compound, and we shall see presently that this defect is exaggerated as we ascend in the series. It is difficult to understand how mineralogists can do without the aid which this ratio affords in the determination of species; and it appears to me to be of the utmost importance for the interests of practical mineralogy, that any formulæ now employed should be capable of exhibiting the above ratio as clearly as those written in the old way did. I am well aware that in order to do this it would *appear* to be necessary to surrender typical formulæ altogether; but it will be

* In the following pages I have adopted, for the sake of harmony, the nomenclature and order of description of the silicates which Professor Dana employs in the new edition of his 'Mineralogy.' Dana calls the normal salt a *unisilicate*, and the second compound a *bisilicate*.

presently seen that no such sacrifice is really needed. I have little doubt that if a reasonable compromise could be effected, the transition from the notation of the old to that of the new system would become so easy that many mineralogists might be induced to adopt the latter who are now opposed to the change. I hope to show presently that this may be done in a satisfactory manner.

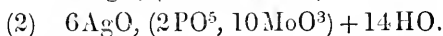
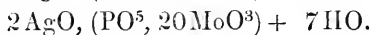
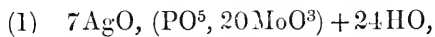
It might be objected that the exhibition of the oxygen ratio of a silicate by a formula is not an accurate statement of fact—that the oxygen acts merely in binding together the acidific and basic elements of the compound, and that the amount of oxygen employed in this way can be only measured by the atomicities of the principal constituents of the salt. To this it might be fairly replied that but little is *really* known of the function of the oxygen contained in a silicate; and though the above statements may be true, all the decompositions with which the mineralogist is acquainted lead him to the general conclusion that each atom of silicon ($=28$) in a silicate has two of the atoms of oxygen more intimately associated with it than the next one. As a striking example of such a decomposition, we might instance the direct liberation of SiO^2 on fusion of an anhydrous silicate with the residue of the ignition of microcosmic salt. It appears to me to be quite as reasonable to separate the oxygen of a silicate into two sections in accordance with such a decomposition, as to divide the oxygen into portions according as part may be supposed to bind together the molecular edifice, and the residue not to be so engaged*. In the absence of distinct proof to the contrary, the utility of the former course would appear to render its adoption preferable.

Accepting SiO^2 ($\text{Si} = 28$) instead of SiO^3 ($\text{Si} = 21$) for silica as the constitution of that body, which has been established beyond a doubt by the recent admirable researches of MM. Friedel and Crafts†, I take, *in a restricted sense*, the view now generally admitted by chemists, viz. that silicic anhydride is capable of almost indefinite polymeric modification; that is to say, we may have not only SiO^2 , but Si^2O^4 , Si^3O^6 , Si^4O^8 , &c., all being simple multiples of the primary SiO^2 . The chief evidence in favour of this theory is afforded by the composition of some of the silicic hydrates, the constitution of which can only be satisfactorily explained on the above view.

* It is necessary to state distinctly here, that in taking this view of the matter I in no way seek to represent that the molecule, in its normal state, is made up of a metallic oxide and silica according to the plan of Berzelius; and it will be seen that I guard against such an idea in the construction of the formulæ adopted, which shows the mutual dependence of the elements constituting a compound.

† *Ann. de Chim. et de Phys.* (4) vol. ix. p. 5.

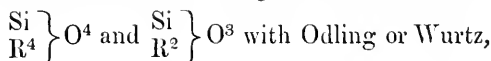
In addition to this, a simple explanation is also afforded of the causes of the great variation in the characters of silica, whether obtained native or prepared artificially. I would further suggest that the recent researches of M. Debray* on the phospho-molybdic acids bear very strongly upon the same point. But very recently some particularly well-defined and crystallized compounds of phosphoric and molybdic acids have been discovered by M. Debray, the composition of which shows that molybdic acid is capable of undergoing an amount of molecular condensation of which we had previously no conception. This is best illustrated by the formulæ of the silver-salts of two of these phospho-molybdic acids. The old notation is that employed by the author.



It would thus appear that we have not only MoO^3 , but at least two other polymers of this— Mo^5O^{15} and $\text{Mo}^{20}\text{O}^{60}$. It is well worthy of remark that the first of these two bodies can be obtained only from alkaline solutions, and the second only from acid liquids, and one modification can be speedily changed into the other by simply realizing either condition. Tungstic acid has been found to possess similar power of atomic condensation.

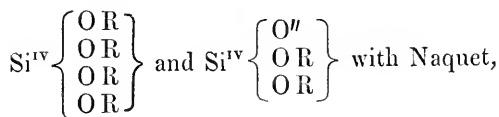
Molybdic, tungstic, and silicic anhydrides and acids are well known to present many chemical analogies; the proof of this power of condensation in the cases of molybdic and tungstic acids, combined with our previous knowledge of the silicic hydrates and of silica itself, warrants us in attributing the same power to silicic acid, more particularly when we shall find the composition of the silicates and their relations to be easily explicable on such a view. In reality, however, this is nothing more than the adaptation of one of the well-known principles of organic chemistry relating to the condensation of radicals—the mode of generation of compounds of this order being thus precisely stated by M. Wurtz† to be “based on the one hand upon the principle of the *accumulation of polyatomic radicals*, and on the other hand upon the *successive dehydration* which may be undergone by hydrates containing polyatomic radicals.”

The plan upon which I construct the formula of the silicates is as follows:—Instead of writing a unisilicate or a bisilicate

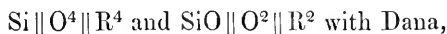


* Chemical News, vol. xvii. p. 183.

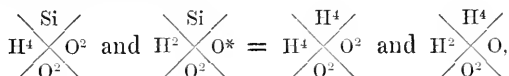
† Crookes's translation of Wurtz's 'Modern Chemical Philosophy.'



or



I employ the following form, constructed, as will be readily seen, on a modified water-type,



1 : 1 and 1 : 2 being the oxygen ratios.

The use of this construction enables me to avoid the anomaly already referred to. This plan is also adopted in order to show the mutual dependence of the elements constituting the molecule of a silicate, the mode in which it is capable of breaking up under the influence of certain reagents, and the oxygen ratio of the compound, and, further, for the purpose of clearly exhibiting the relations of the salt of a condensed acid to its analogues amongst the lower terms of the series. It appears to me that these advantages are gained by the above construction, without involving our acceptance of any particular theory as to the *absolute structure* of the molecule.

In adopting this plan in the cases of the silicates the formulæ of which are given below, I seek to show much more than the mere composition of some of the minerals belonging to the several groups. If we disregard the minor subdivisions, it is shown that all the anhydrous mineral silicates may be divided into two great classes—(1) the silicates which contain in each molecule the elements of SiO^2 only, and (2) those which contain but $3(\text{SiO}^2) = \text{Si}^3 \dagger \text{O}^6$, or a multiple of this quantity.

When the force of this division is realized, I believe that much of the difficulty at present attending the study of the mineral silicates will be found to have disappeared; moreover the relations of groups of minerals become quickly evident when their formulæ are constructed in accordance with the principles above stated.

* It is obvious that this is equivalent to writing the first hydrate $\begin{array}{c} \text{S} \\ \text{H}^4 \times \\ \text{O}^1 \end{array}$ and the second $\begin{array}{c} \text{Si} \\ \text{H}^2 \times \\ \text{O}^3 \end{array}$, but by the construction adopted in the text it

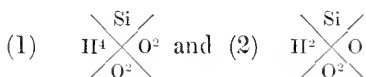
becomes possible to express the "harmonies of variations" of the molecule.

† Hereafter in the text Si (=28) is used to represent the silicon as it occurs in SiO^2 , and **Si** (=84) as in SiO^6 , the condensed anhydride. The convenience of this distinction is very considerable.

I am well aware that the mode of regarding the native silicates proposed in this paper is not likely to be considered with much favour by those chemists who, with Von Kobell*, dislike the idea of innovation, or others who, with Naquet†, appear to be disposed to place no limit to the power of condensation and successive dehydration of the polysilicic acids. I have therefore sought for such experimental evidence as might be expected to throw any light upon the subject, and hope soon to be able to communicate the results of some experiments in a new direction which promise to give a clue to the modes to be adopted in distinguishing the different conditions of SiO^2 throughout some of their compounds. Much independent evidence, however, is forthcoming; and I will now proceed to show that the composition of the natural and artificial hydrates of SiO^2 can be easily and simply explained in accordance with the principles already stated—and then, that the relations of the native anhydrous salts become apparent when their formulæ are treated in a similar manner.

SILICIC HYDRATES.

1. *Hydrates of SiO^2 .*—Frankland‡ states that the normal hydrate (1) is the body separated from a soluble silicate by treatment with a stronger acid. Professor Graham's compound (2) corresponds well to the formula of the dibasic acid§.



In the absence of distinct evidence to the contrary, we may regard the above as silicic hydrates uncondensed. All the salts of the uncondensed SiO^2 are formed on one or the other of the above types.

We have now the second great division of the silicic hydrates and metallic salts, *i. e.* that comprising the hydrates of $\text{SiO}^6 = 3(\text{SiO}^2)$, or a simple multiple of this.

2. *Hydrates of SiO^6 .*—(a) First amongst these hydrates stands that prepared and analyzed by Doveri||, and which may be well

represented by the formula $\begin{array}{c} \text{Si} \\ \diagup \quad \diagdown \\ \text{H}^2 \quad \text{O} \\ \diagdown \quad \diagup \\ \text{O}^6 \end{array}$. This corresponds precisely

to Graham's hydrate of the uncondensed acid.

* *Journal für praktische Chemie*, 1859, p. 168.

† *Op. cit.*

‡ Lecture Notes, p. 101.

§ *Philosophical Transactions*, 1861, p. 204.

|| *Ann. de Chim. et de Phys.* (3) vol. xvi. p. 129.

(b) The next hydrates are those of 2SiO^6 ; and Salvétat's mineral, Randanite*, $\text{H}^2 \begin{array}{c} \text{Si}^2 \\ \times \\ \text{O}^{12} \end{array}$, exactly corresponds to one hydrate of this body. Fuchs's compound† is probably another hydrate of the same acid, viz. $\text{H}^6 \begin{array}{c} \text{Si}^2 \\ \times \\ \text{O}^{12} \end{array}$; and we may have Ebelmen's compound‡ as a third of the series, $\text{H}^{12} \begin{array}{c} \text{Si}^2 \\ \times \\ \text{O}^{12} \end{array}$.

(c) In this subdivision we have the hydrates of 3SiO^6 , which are few in number. Some varieties of semiopal appear to consist of the first hydrate, $\text{H}^2 \begin{array}{c} \text{Si}^3 \\ \times \\ \text{O}^{18} \end{array}$. It will be seen further on that orthoclase can be best represented as constructed on the type of the hydrate $\text{H}^{12} \begin{array}{c} \text{Si}^3 \\ \times \\ \text{O}^{18} \end{array}$.

It is a singular fact, and one well worthy of remark in this connexion, that the proportion of water found in all the native hydrates of silica hitherto analyzed is such that the specimens containing most water would be best represented by the formula

$\text{H}^2 \begin{array}{c} \text{Si} \\ \times \\ \text{O}^6 \end{array}$, and those containing least water fall into the *c* series above, as $\text{H}^2 \begin{array}{c} \text{Si}^3 \\ \times \\ \text{O}^{18} \end{array}$, while Randanite, $\text{H}^2 \begin{array}{c} \text{Si}^2 \\ \times \\ \text{O}^{12} \end{array}$, is the intermediate term required. It is quite true that we know little of the function of the water in these compounds; but the above proportion would appear to be the result of more than mere accident, especially when viewed in connexion with the relations exhibited below between some of the silicic hydrates and their corresponding metallic salts.

* Quoted in Dana's 'Descriptive Mineralogy,' new edition, 1868, p. 200. The above is the formula of the mineral dried at 100°C .

† *Ann. der Chem. und Pharm.* vol. xxxii. p. 119.

‡ *Ann. de Chim. et de Phys.* (3) vol. xvi. p. 129.

GENERAL CLASSIFICATION OF THE ANHYDROUS SILICATES.

Acid or Bisilicates.

	New atomic weights.	Old atomic weights*.	Oxygen ratio.
Class 1. Type of class .	$\text{H}^2 \text{Si} \text{O}^2$	$\text{H} \text{Si} \text{O}^2$	1 : 2.
Class 2. „	$\text{H}^6 \text{Si} \text{O}^3$	$\text{H}^3 \text{Si} \text{O}^3$	1 : 2.
Class 3. „	$\text{H}^6 \text{Si}^2 \text{O}^3$	$\text{H}^3 \text{Si}^2 \text{O}^3$	1 : 4.

All the acid silicates can be well represented on either of the above three types.

Normal or Unisilicates.

Class 1. Type . . .	$\text{H}^1 \text{Si} \text{O}^2$	$\text{H}^2 \text{Si} \text{O}^2$	1 : 1.
Class 2. „ . . .	$\text{H}^{12} \text{Si} \text{O}^6$	$\text{H}^6 \text{Si} \text{O}^6$	1 : 1.
Class 3. „ . . .	$\text{H}^{12} \text{Si}^2 \text{O}^6$	$\text{H}^6 \text{Si}^2 \text{O}^6$	1 : 2.
Class 4. „ . . .	$\text{H}^{12} \text{Si}^3 \text{O}^6$	$\text{H}^6 \text{Si}^3 \text{O}^6$	1 : 3.

Basic Silicates.

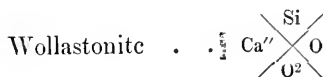
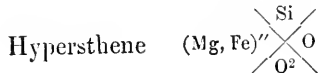
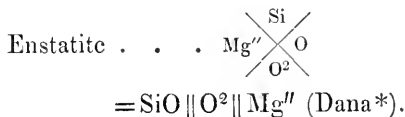
Class 1. Type . . .	$\text{H}^{16} \text{Si} \text{O}^4$	$\text{H}^4 \text{Si} \text{O}^4$	4 : 3.
Class 2. „ . . .	$\text{H}^{14} \text{Si} \text{O}^3$	$\text{H}^9 \text{Si} \text{O}^3$	3 : 2.
Class 3. „ . . .	$\text{H}^{24} \text{Si} \text{O}^{12}$	$\text{H}^{12} \text{Si} \text{O}^{12}$	2 : 1.

* By contrasting the old and new type-formulae, when constructed on the plan proposed in this paper, it will be seen how very easy the transition from the equivalent to the atomic notation really is.

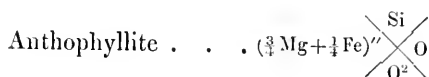
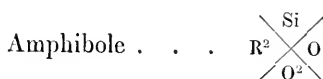
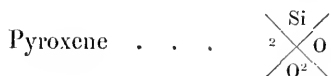
I am well aware that modern views on the molecular construction of condensed compounds would oblige us to deny the possibility of obtaining *some* of the above hydrates in the free state. It need scarcely be remarked that though this is admittedly the case with such unstable bodies as the polysilicic hydrates, yet it is well known that the metallic salts of condensed acids may exist though the isolation of their corresponding hydrates would be a matter of doubt, or (according to present ideas) an impossibility. Whatever views on the subject may ultimately obtain acceptance, they are not likely to alter the above general modes of expression for the atomic ratios of the best known native silicates. In the cases of the basic or subsilicates, it is shown above that they correspond to one or other of three hydrates of the first stage of condensation of SiO^2 dealt with in this paper, viz. SiO^6 . In this way their relations can be clearly brought out, though the cause of the peculiar ratio of $\text{R}(=\text{H})$ to Si is difficult to understand. The normal or unisilicates ought to be the most highly basic of these salts, and yet we find the subsilicate of Class 3 containing twice as much basic element as an unisilicate. According to Dr. Odling this anomaly may be explained thus:—"If we consider alumina as always basic, then there are silicates" (such as those above typified) "in which the basicity exceeds that of the tetrabasic group; but the basic or acidulous function of alumina is yet *sub judice*; and if we exclude aluminous minerals, we shall find that the tetrabasic are the most highly basic of all known silicates, natural or artificial." These remarks are specially directed to the proof of the conformity of silicic to other acids, but they obviously apply forcibly to the question of the constitution of the subsilicates on the types I have given above.

Bisilicates.

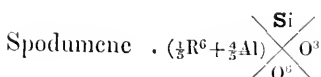
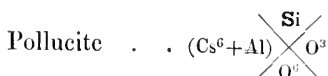
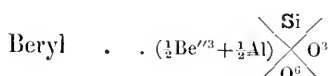
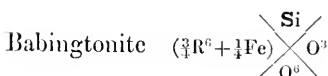
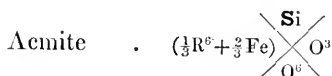
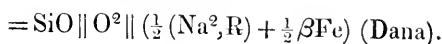
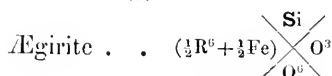
Class I.



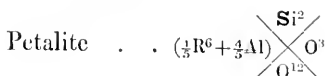
* With each class I give Dana's new formula for the first member of the group.



Class 2.



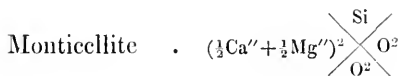
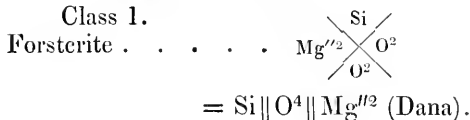
Class 3.



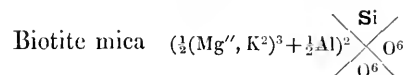
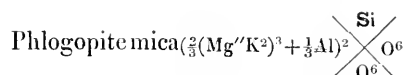
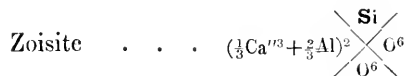
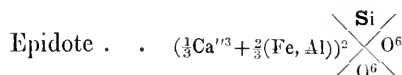
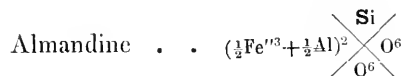
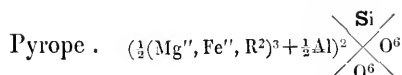
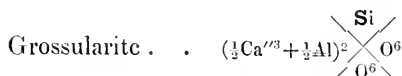
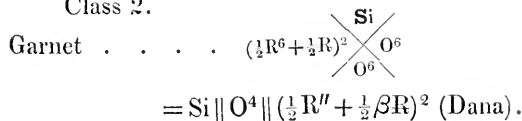
The last is the only member of Class 3 occurring in this division. The relation of petalite to spodumene and other members of the section is thus clearly shown by the formulæ.

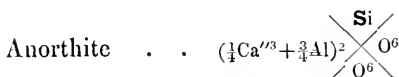
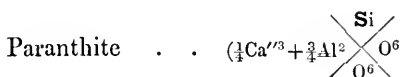
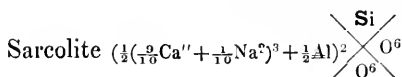
Normal or Unisilicates.

Class 1.

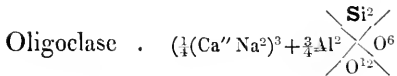
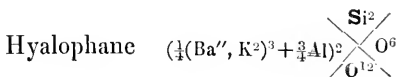
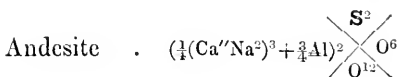
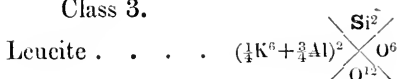


Class 2.

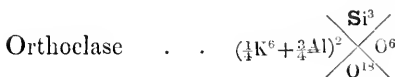
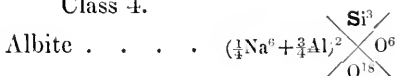




Class 3.



Class 4.



As these lists of minerals are only intended to illustrate the application of the principle of classification proposed in this paper, I have carefully avoided entering upon doubtful questions by expressing the constitution of silicates which have been hitherto insufficiently examined.

The above arrangement of the formulæ renders remarkably evident the relations existing between the members of the felspar and other families. This group of minerals illustrates the mode of accumulation of the anhydride SiO^6 upon itself without otherwise disturbing the arrangement of the molecule; and the composition of these silicates appears to me forcibly to testify that *the normal rate of condensation of the molecule of SiO^2 is three, or some simple multiple of that number.*

It is a well-known fact that anorthite and leucite are two im-

portant feldspars of the volcanic rocks, just as albite and orthoclase are of the granitic rocks. Of these four (and other intermediate compounds), however, leucite and orthoclase are the most directly comparable, being both potassium and aluminium feldspars, evidently formed on precisely the same type, yet differing by the compound molecule SiO^6 . It now becomes a matter of much interest to attempt an explanation of this remarkable and yet methodic variation in the acidific portion of these feldspars. With the assistance derived from some known properties of the silicic hydrates, and the Rev. Professor Haughton's valuable translation of Durocher's memoir on Comparative Petrology*, I think it is possible to understand the conditions under which one or other of these feldspars can be formed, and the mode of production of either. We are already well aware of the fact that leucite is convertible into orthoclase by removal of potash and alumina by aqueous action; but this information gives little aid in the direction we require.

It has been already stated that the silicic hydrates are very unstable bodies, desiccation of a hydrate, $\text{H}^2 \times \frac{\text{Si}}{\text{O}^2}$, at 100°C .

being sufficient to convert it into the polysilicic hydrate $\text{H}^2 \times \frac{\text{Si}}{\text{O}^6}$.

Again, the mineral Randanite of Salvétat, already several times referred to, when dried completely at ordinary temperatures has

its composition represented by the formula $\text{H}^2 \times \frac{\text{Si}}{\text{O}^6}$; on drying at 100°C . for a long time, however, its composition then is

$\text{H}^2 \times \frac{\text{Si}^2}{\text{O}^{12}}$. Possibly, on still further heating, another compound

would be obtained. It is easy to understand, therefore, how a *silicic acid* may yield a *polysilicic acid* by the simple loss of water. But the metallic base of a silicate cannot be got rid of so easily. Heating of the compound alone cannot alter it, notwithstanding the marked tendency of a silicate of a low type to pass into a polysilicate; for as the temperature increases, this tendency likewise grows. Assuming that a typical silicate is capable of existing in a semifused magma, and that its formula is

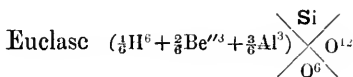
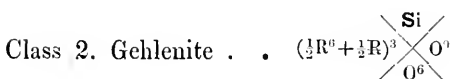
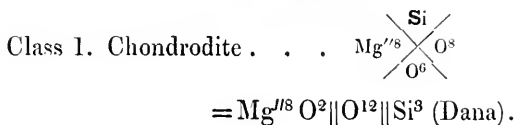
$(\frac{1}{4}\text{K}^6 + \frac{3}{4}\text{Al})^2 \times \frac{\text{Si}}{\text{O}^6}$, it obviously cannot lose its basic elements,

* Haughton's 'Manual of Geology,' 1866, p. 16. Durocher's important essay is here given in full, and contributes probably the best aid to be found to the study of the paragenesis of minerals.

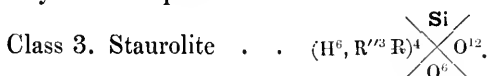
unless some stronger affinity than that of the SiO^6 comes into play; yet its tendency is to pass into leucite, which mineral would result from the addition of SiO^6 to the above group. Though it cannot undergo the change *by loss of base*, it can, and no doubt does, *by increment of acidulous radical*; it will seize any silica which the necessity of the compound molecule enables it to displace from feeble combinations. The result of this is that the SiO^6 is, or its elements are, condensed within the molecule of the silicate, the tendency to accumulation of the polyatomic radical is *partially* satisfied, and *leucite* is the result. Yet this is supposed to take place in the magma which on cooling gives the volcanic rocks; and Durocher tells us, and numerous analyses prove amply, that these rocks are essentially *basic*, and comparatively poor in silica. It would appear, then, that the tendency of the acid radical of the ideal silicate to reach the second stage of condensation is sufficient to enable it to overcome the opposing influences around it in order that leucite may be produced; but the cumulative power is not enough to enable it to pass into orthoclase by the appropriation and condensation of the silica of other compounds. When we come to the granitic rocks, however, no such paucity of silica is observed; on the contrary, we have this body largely in excess, and the necessary consequence is, that in these acid rocks we meet with orthoclase, a mineral in which the acid radical appears to have reached its greatest proportion, and the silica to have completely satisfied its tendency to accumulation within the molecule.

In concluding this section, I may remark that the third stage of condensation of SiO^6 is the highest which appears to have been reached in any mineral. Dana* regards the presence of potassium and sodium in a felspar as the condition which determines the greater or less proportion of silica in such a compound. There appears to be much truth in this; but I think the genesis of a mineral, as instanced above in examining the relations of leucite and orthoclase, should also receive full attention. When this important point is weighed in connexion with the explanation above offered of the causes which have probably operated in the production of one or other mineral, we see still more clearly than before the profound isotypism of the felspar family; and not only this, but we have distinctly brought out the peculiar rate of condensation of silicon within the molecule of some silicates which it has been the object of the present paper to call attention to as affording a simple means of classifying the salts of normal and polysilicic acids.

* American Journal of Science, vol. xlv. p. 401.

Basic or Subsilitates.

Probably the composition of datholite and tourmaline can be also best exhibited on a similar plan, if we regard the boric anhydride as a basic constituent. It need scarcely be said that this is a very doubtful point.



It will have been already noticed that I have rarely dealt with the formulæ of hydrous metallic silicates. So little is really known of the function of the water contained in them, that it seemed undesirable to dwell upon their atomic formulæ at present; at the same time it is necessary to say that the composition of large numbers of them can be easily represented in accordance with the plan proposed in this paper. This is especially the case with the interesting family of the zeolites, the relations of which to that of the felspars can be then clearly traced.

In the preceding pages I have done little more than note down the main points which I sought to give prominence to. In every section numerous cases occur which admit of extended treatment, yet these have not been dwelt upon, nor have more formulæ been given than were absolutely necessary to facilitate explanation. This course seemed preferable, as it then became possible to give an outline of the subject without extending this paper beyond due limits.

In conclusion, I may sum up the main points of this paper
Phil. Mag. S. 4. Vol. 36. No. 243. Oct. 1868. U

in the following terms. Some evidence has been brought forward to show :—

(1) That by an alteration in the construction of formulæ, the atomic ratios of the silicates may be easily expressed in accordance with the principles of modern chemistry, while some practical advantages are gained, and those attending the use of the old formulæ are retained.

(2) That the constitution and relations of the silicic hydrates and salts are best understood by regarding them as (a) containing the elements of the primary SiO^2 , or (b) a polymer of this.

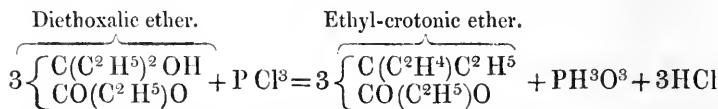
(3) That the rate of condensation of SiO^2 within the molecule of a native anhydrous salt is represented by the number 3, or a multiple of this by 2 or 3 *only* = SiO^2 , SiO^6 , 2SiO^6 , 3SiO^6 ; that this alteration can take place without interfering with the general type of the compound, and that the passage from one stage of accumulation to another can be easily explained by reference to the known properties of normal and polysilicic hydrates.

It is also shown that the silica, hitherto regarded either as *basic* or as *accessory* in such minerals as the felspars, is present really as the result of the *partial or complete satisfaction* of the tendency of SiO^6 to pass successively into 2SiO^6 and 3SiO^6 , the chemical conditions of the mass in which the mineral may have been formed determining the degree of condensation within the molecule.

XXXVII. *Limited Oxidation of Ethyl-crotonic Acid.*

By ERNEST THEOPHRON CHAPMAN and MILES H. SMITH*.

THE ethyl-crotonic acid employed in the following experiments was prepared from diethoxalate of ethyl in the manner described by Frankland and Duppa. The process consists in adding trichloride of phosphorus to the diethoxalic ether†. The ether loses one atom of water, thus :—



At the same time red amorphous phosphorus is deposited. This, we suppose, owes its origin to an action between the phosphorous

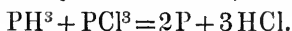
* Communicated by the Authors.

† For the preparation of this ether see "Synthetical Researches on Acids of the Lactic Series," by Frankland and Duppa, Phil. Trans. for 1866, p. 309; and also Journal of the Chemical Society, April 1867, where an improved method of preparing the ether is described by us.

acid and the excess of trichloride of phosphorus. The phosphorous acid has a tendency to split up into phosphuretted hydrogen and phosphorous acid, thus :—



And P H^3 , acting on trichloride of phosphorus, is known to yield phosphorous acid and hydrochloric acid, thus :—



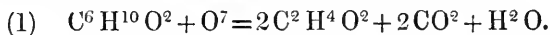
At any rate more than the theoretical quantity of trichloride is required to convert the ether into the new compound.

Ethyl- or ethocrotonic* ether and acid have been so fully described that we have little or nothing to remark with regard to them. We titrated the ether with the following result :—

4·263 grms. required alcoholic potash equivalent to ·96423 of potassium. Therefore 100 parts of the ether neutralized 27·31 parts of potassium ; theory requires 27·53. The ether was therefore pure.

The acid was prepared from the ether, and subjected to the action of bichromate of potash and sulphuric acid. An experiment on an unknown quantity of substance proved that acetic and carbonic acids were the sole products of the oxidation. This determination was made by methods which have been fully described elsewhere†.

The only point remaining to be ascertained, therefore, was, Does the ethyl-crotonic acid split up into two atoms of acetic acid and two of carbonic, or into one atom of acetic acid and four of carbonic ? To ascertain this, and at the same time to check the non-quantitative portion of the work, a determination of the amount of oxygen required to effect the change was made by the method already described by one of us‡. If the decomposition took place so as to yield two atoms of acetic acid and two of carbonic acid, seven atoms of oxygen only would be required, whereas the other decomposition would require eleven atoms.



A known weight of the acid was sealed up with a known quantity of a standard solution of bichromate in dilute sulphuric acid. The tube was heated for some hours in the steam-bath. It was then cooled and opened, and its contents transferred to a small flask. The tube was washed out, and the washings also poured into the same flask. The flask was heated in the water-bath, and

* Frankland and Duppa, Chem. Soc. Journ. 1865, p. 133.

† Chapman and Thorp, Journ. of Chem. Soc. p. 477 (1866).

‡ Chapman, Journ. of Chem. Soc. 1867, p. 227.

then, whilst still hot, placed under the receiver of the air-pump. Of course this caused the contents of the flask to boil, and thereby effectually expelled any carbonic acid that might be dissolved in the liquid. The strength of the standard chromic solution had previously been ascertained by causing it to act on excess of oxalic acid. The carbonic acid so liberated was dried, absorbed by potash, and weighed. Two equivalents of carbonic acid correspond to one of available oxygen.

The chromic acid which had not been reduced by the ethyl-crotonic acid was now estimated in the same way, and the carbonic acid so obtained deducted from the amount which the known quantity of chromic solution would have liberated had no portion of it been reduced. From the difference it is easy to calculate how much oxygen the acid had consumed.

The details of the operation are as follows:—

Strength of chromic solution. 100 grms. of the solution liberated 7·5182 grms. of carbonic acid from oxalic acid.

Amount of chromic solution employed 51·02 grms.

Ethyl-crotonic acid employed 4316 gm.

Carbonic acid obtainable from unaltered chromic solution	3·8358
--	--------

Carbonic acid obtained after digestion	1·4496
--	--------

Difference, due to oxygen consumed	2·3862
--	--------

Two equivalents of carbonic acid correspond to one of oxygen.
Therefore $2\text{CO}_2 = 88 : \text{O} = 16 :: 2·3862 : x$,

$$x = \cdot 133854.$$

Dividing this by the substance taken, we learn the percentage of oxygen consumed, viz. 100·47

Theory for seven equivalents 98·24

Theory for eleven equivalents 154·38

Therefore equation (1) is evidently correct.

The above work was the first step in a proposed investigation, by oxidation, of the acids of the acrylic series. It was intended to collect these acids and treat them in a similar manner; but the difficulty of obtaining any of these acids, excepting those which could be formed synthetically from the corresponding secondary lactic acids, proved so great that the investigation was temporarily abandoned. Should opportunity offer we intend to resume it.

Laboratory, London Institution.

XXXVIII. *On the Polarization of Heat radiated from the Surfaces of Liquids.* By Professor MAGNUS*.

SINCE my communication of March 5 last†, containing the results of an investigation on the polarization of the heat radiated by solid bodies when heated to 100° C., I have been occupied with the examination of the heat radiated by liquids. In these experiments it was impossible to employ the same form of apparatus as for solids, because it was necessary for the radiating surface to be horizontal. The whole portion of the apparatus containing the reflecting mirror, and the box containing it, was therefore placed upon a plane inclined at an angle of 35° to the horizon. The radiating vessel consisted of a completely closed box 12 centims. broad, 21 centims. long, and 4 centims. high, which was maintained at a temperature of 100° C. by a current of steam. This box was placed in a horizontal position; and upon it was fastened a tray of sheet iron of equal length and breadth, but only 1 centim. deep. The radiating liquid was placed in this tray.

In order to test this arrangement, a plate of polished tin plate was first introduced into the tray instead of the liquid. The polarization of the heat radiated by this plate was found to be almost exactly the same as when the former arrangement was employed‡, namely 25.8 and 26.0 per cent. in two experiments. Also when a plate of transparent glass was placed in the tray instead of a liquid, almost the same value for the polarization was found as previously§, namely 11.9, 11.7, and 9.82 per cent. in three experiments. Consequently this modification of the apparatus gave results as trustworthy as those given by the former one. Such verification of the apparatus was essential, because it was not possible to give the radiant surface various azimuths as with radiating solids.

Only such liquids could be used as boil at a temperature above 100° C.; and there are only a few of these which do not, at this temperature, evaporate rapidly and give rise to clouds in such a manner as to obstruct the radiation. Consequently the number of liquids which could be employed was exceedingly small. Neither amyl-alcohol, boiling at 120° C., nor aniline, boiling at 180° C., could be used, on account of the formation of these clouds. The

* Translated by Frederick Guthrie, F.R.S.E., from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*. April 1868, p. 249.

† Phil. Mag. September 1868.

‡ Ibid.

§ Ibid.

experiments were therefore confined to mercury, fatty oils, glycerine, melted resins, paraffine, and such like.

Liquids present also an especial difficulty in consequence of their surfaces, when the liquids are warmed, not being so flat as they are at ordinary temperatures. In all cases when we examine a liquid which is of a somewhat higher temperature than the surrounding air, we find the image reflected from it to be no longer completely well defined. Heat, in fact, is continually given off from the surface, the cooler portions sink to make room for the warmer portions which rise; and hence are produced movements, small indeed, but which, in consequence of their taking place to a greater extent at one part than at another, give rise to different motions of the surface at different places, and do not allow it to retain its smoothness.

Consequently too much reliance must not be placed upon the numbers. Nevertheless it was found that a portion of the heat radiated from all the liquids examined was polarized. Mercury showed a polarization of 32·0 and 32·2 per cent., a value at least as great as that found for solid metals*. Other liquids, on the contrary, gave less values.

Rape-oil . . .	{ 6·17 per cent.
	{ 5·11 „
Colophony . .	7·26 „
White wax . .	7·3 „
Glycerine . .	5·61 „
Paraffine . .	5·0 „

In order to see if the diathermancy of the liquid had influenced the results, the bottom of the tray containing the liquid was covered with a bright metal plate, with a blackened metal plate, and with lampblack, in succession, before pouring in the liquid. Under each of these conditions the same values were found for the same liquids. It hence appears that the radiating-power of the bottom of the tray has no influence in these experiments, and that consequently the liquids examined are athermanous for heat of the low temperature here employed.

The following is a Table of the values for all the substances hitherto examined:—

* Phil. Mag. *loc. cit.*

Substance.	Polarized portion of the heat radiated at an angle of 35°.
Tinned iron	27·6 per cent.
Copper	22·4 „
Aluminium	28·5 „
Mercury	32·0 „
Transparent glass . .	10·4 „
Black glass	12·4 „
Rape-oil	5·64 „
Colophony	7·26 „
White wax	7·3 „
Glycerine	5·61 „
Paraffine	5·0 „

From these results we are justified in concluding that all substances, whether solid or liquid, radiate from their flat surfaces heat which is partly polarized when it makes an angle of about 35° with the surface.

XXXIX. *Note on Successive Involutes to a Circle* *.

By J. J. SYLVESTER †.

IT is surprising that the families and groups of families of forms capable of being educed by successive involution from a circle should not have attracted the attention of geometers. I find, if any, not more than a passing allusion to their existence in Dr. Salmon's 'Higher Plane Curves,' in the 'Integral Calculus' of Mr. Todhunter, or in the memoirs of the late Dr. Whewell in the Cambridge Philosophical Transactions (vols. viii and ix.), although these latter are exclusively devoted to the study of various curves of mechanical and kinematical origin by aid of the so-called intrinsic equation, which is in fact the natural expression of, and key to, the properties of such like curves. And yet this form of equation almost instantaneously furnishes the general polar equation to the entire system of circular involutes, and exhibits at once their leading properties ‡.

* The germ of this Note was communicated to the Mathematical Section of the British Association at the Norwich Meeting.

† Communicated by the Author.

‡ Professor Rankine and Mr. Merrifield have made a useful application of the *second* involute of the circle to the calculation of the stability of the finite solution of vessels. In the Turkey carpet under my eyes whilst this is being written, I perceive graceful and complicated figures of winding and intersecting scrolls and convolutions, which render it, I think, not at all improbable that the successive involutes of the circle would furnish or suggest many patterns available for decorative purposes: the enormous variety of each kind of involute, which of course increases with the order of derivation, adds to the probability of this conjecture.

Foremost amongst these stands the algebraical form of equation (and that a quantie), which connects not only the are, but also the squared radius vector with the angle of contingence, and consequently the two former with one another. In a marvellous and, so to say, transcendental fashion, these curves participate in the nature of algebraical curves—their apses, cusps, and points of retrocession being counted by the order of the involution, and becoming imaginary in pairs.

I need hardly say that by a second involute I mean an involute of the first by a third, an involute of a second, and so on in continual progression. To any given curve all its first involutes form a system of parallel curves, so that in general the number of *form*-parameters to a curve will be augmented by i when we pass to its general involute of the i th order. In the case of the circle, however, owing to its homogeneity, the first involute, like the curve itself, contains only one *form*-parameter (it being, in other words, a property of the first involute, that when rotated round a certain point, the curves so generated continue always parallel to each other); and so the number of *form*-parameters in the general i th involute will contain i , and not $i+1$ parameters, as the general formula would require.

I shall use ϕ , s , r , θ to denote the angle of contingence, are, radius vector, and vectorial angle of the curves under consideration.

Starting from the circle $s = a\phi$, a set of corresponding successive involutes will, as is well known, be represented by

$$s_1 = a\phi^2 + b\phi; \quad s_2 = \frac{a\phi^3}{6} + \frac{b\phi^2}{2} + c\phi,$$

and so on, according to the obvious law

$$s_i = \int d\phi \cdot s_{i-1}.$$

Now in general for any *curve* whatever, if we call p the perpendicular on the tangent from an arbitrary pole, q the projection of the radius on the tangent, we have

$$q = -\frac{dp}{d\phi}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

also

$$p + \frac{d^2p}{d\phi^2} = \frac{ds}{d\phi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$p^2 + \left(\frac{dp}{d\phi}\right)^2 r^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

These equations are of course not new*; they are given by

* We have only to take P, P', two consecutive points, and on PP', P'T,

Mr. Todhunter in the later editions of his 'Integral Calculus,' accompanied with a reference to another English treatise, from

the tangents at P, P', draw perpendiculars from an arbitrary point O, and we obtain at once, by inspection,

$$\delta p = -q \delta \phi, \quad \delta q + \delta s = p \delta \phi,$$

whence

$$q = -\frac{dp}{d\phi}, \quad p + \frac{d^2p}{d\phi^2} = \frac{ds}{d\phi}.$$

Or, again, proceeding analytically, we have

$$x - A = \int ds \cos \phi, \quad y - B = \int ds \sin \phi;$$

whence, integrating by parts,

$$x - A = G \cos \phi - G' \sin \phi,$$

$$y - B = G \sin \phi + G' \cos \phi,$$

where

$$G = s' - s''' \dots; \quad G' = s'' - s'''' \dots;$$

whence

$$r = G^2 + G'^2 \quad \text{and} \quad G + G'' = \frac{ds}{d\phi}.$$

From which also we may deduce

$$q = r \frac{dr}{ds} = G', \quad p^2 = r^2 - q^2 = G^2.$$

This last demonstration would at first sight seem to be only valid for the case of the G series coming to an end, i. e. of $\frac{ds}{d\phi}$ being a rational integral function in ϕ ; but it would be quite legitimate to infer at once from it the *universality* of the equations above written connecting r^2 with s and ϕ ; for we may write down the general differential equation of the second order

$$d \cos^{-1} \frac{dr}{ds} - d\theta = d\phi,$$

i. e.

$$\frac{d \cdot \frac{dr}{ds}}{\sqrt{(ds)^2 - (dr)^2}} - \frac{\sqrt{ds^2 - dr^2}}{r} = d\phi,$$

in which $\frac{ds}{d\phi}$ may be considered as given, and r or r^2 to be determined.

The equations in question, having been shown to be true for a form $\frac{ds}{d\phi}$ containing an indefinite number of arbitrary constants, evidently can only amount to a transformation of, and may be used in supersession of the equation last written. It may be worth while to set out this latter under a more familiar form of notation. If, then, we use y for r and x for ϕ ,

and call $\frac{ds}{d\phi} = X$ (any function of x), it becomes

$$\frac{-Xy'' + X'y'}{X\sqrt{X^2 - y'^2}} + \frac{\sqrt{X^2 - y'^2}}{y} = 1,$$

an apparently very complicated form of equation, but admitting of the simple solution $y^2 = u^2 + u'^2$, where u satisfies the linear differential equation $u + u'' = X$.

which he has taken them; but in themselves, easily as they can be obtained, they contain the whole theory of the remarkable curves to which this note refers. In the case before, $\frac{ds}{d\phi} = F\phi$, where F is (a quantic in, *i. e.*) a rational integral function of ϕ . Hence we have for the solution of (1),

$$p = F - F'' + F''' \dots + A \cos \phi + B \sin \phi,$$

$$p = F''' - F'''' + \dots - A \sin \phi + B \cos \phi;$$

wherefore r^2 is known in terms of F and the arbitrary constants A and B , whose values depend on the position of the origin from which r is reckoned, by a due choice of which they may be made to vanish. One will readily suppose that this eligible position of the pole must be the centre of the generating circle; and the proof is as follows:—

If $r_1, r_2, \dots r_i$ be any radii vectores corresponding to $s_1, s_2, \dots s_i$, it is well known, and follows from the definition of the involute, that

$$r_{i+1}^2 = r_i^2 - 2r_i s_i \frac{dr_i}{ds_i} + s_i^2. \dots \dots (4)$$

Now, suppose that for any number i the origin has been so chosen that

$$r_i^2 = (s_{i-1} - s_{i-3} + \dots)^2 + (s_{i-2} - s_{i-4} + \dots)^2,$$

then

$$r_i \frac{dr_i}{ds_i} = \frac{1}{s_{i-1}} \left\{ (s_{i-1} - s_{i-3} \dots)(s_{i-2} - s_{i-4} \dots) \right\} \\ + (s_{i-2} - s_{i-4} \dots)(s_{i-3} - s_{i-5} \dots) \}$$

$$= s_{i-2} - s_{i-4} + \dots$$

Hence

$$r_{i+1}^2 = (s - s_{i-2} + s_{i-4} \dots)^2 + (s_{i-1} - s_{i-3} + \dots)^2;$$

and the supposed relation, if true for any value i , is true for all superior values; but when the origin is at the centre, $s_1 = a\phi$, $r_1^2 = a^2$; and consequently the equation (4) is true universally*.

* The above result might have been deduced more directly from the equation $\frac{dp_i}{d\phi} = p_{i-1}$, which is true for any curve and its evolute. In fact this paper need never have been written (for all that it contains is a straightforward inference from four equations which may be found scattered up and down in elementary treatises), had it been the custom to regard those equations as forming collectively a connected apparatus. I mean the four following, where the unaccented and accented s and p refer to any curve and its evolute respectively, ϕ being the angle of contingence common in mag-

Thus, then, when the origin is at the centre, whilst for the i th involute to a circle the arc is any quantic $\int d\phi F$ of degree $(i+1)$ in ϕ , r^2 is a quantic in ϕ of degree $2i$ of the particular form $(G\phi)^2 + (G'\phi)^2$, where $G(\phi)$ may be supposed, if we please, to be any quantic of the order i or ϕ^* ; and then $\frac{ds}{d\phi}$ the radius of curvature at s , ϕ , is expressed by $G\phi + G'\phi^\dagger$.

It may be here noticed that substituting for ϕ , $\phi + \lambda$, where λ is arbitrary, amounts only to a rotation of the curve through the angle λ , so that, as regards the intrinsic form of the curve, no generality is sacrificed by imposing one condition upon the coefficients in $G\phi$, or, if we please, in making any of the coefficients in it except the first to vanish.

nitude to the two :—

$$(1) \quad \frac{ds}{d\phi} = p + \frac{d^2p}{d\phi^2}, \quad (2) \quad r^2 = p^2 + \left(\frac{dp}{d\phi}\right)^2,$$

$$(3) \quad s' = \frac{ds}{d\phi}, \quad (4) \quad p' = \frac{dp}{d\phi},$$

the last of them more familiarly known under the form

$$p'^2 = r^2 - p^2.$$

The third and fourth equations show respectively that s and p are each quantics in ϕ ; the first gives the connexion between the constants which enter into these quantics, and the second, combined with the first, the relation between s and r (in other words, the rectification of the curve), that between r and θ (where θ is the vectorial angle) being contained in a fifth equation,

$$\theta = \phi + \sin^{-1} \frac{p}{r}.$$

* Accordingly we see that the spiral of Archimedes, as is well known, is the locus of the feet of the perpendiculars upon the tangents to the first involute from the centre of the circle; and, much more generally, if we substitute for each radius vector in this spiral any given quantic thereof, we obtain the corresponding first pedal to an involute whose order of derivation is the degree of the quantic. *Ex. gr.* by squaring or cubing the radius vector of the spiral of Archimedes (of course leaving the vectorial angle unchanged), we may form the pedal to particular species of the second and third involutes respectively.

† Since the radius of curvature, radius vector, and perpendicular on the tangent arc are all known rational integral functions of the same quantity, it becomes a simple problem of elimination to determine the central force competent to make a body describe an involute of any order to a circle. Thus it will be found that the half-pitch second involute may be described under the action of a central force varying as the inverse cube of the shortest distance from the generating circle. So, again, the first involute may be described under the action of a central force, the component of which in the direction of the tangent to the generating circle (or say the centrifugal force) varies as the inverse cube of this tangent, the centre of force in each case being of course situated at the centre of the circle.

From what precedes, and from the general theory of elimination, it follows that in general the relation between r^2 and s is expressed by a rational integral equation of the degree $(i+1)$ in the former and $2i$ in the latter. But this is subject to an obvious exception in the case of $i=1$; for then, calling $G\phi=a\phi$, we have

$$\frac{ds}{d\phi} = a\phi, \quad s = \frac{a\phi^2}{2} + b, \quad \text{and } r^2 = a^2\phi^2 + a^2 = 2as + (a^2 - 2ab);$$

so that the degrees in r^2 and s are here 1 and 1 in lieu of 2 and 2, as given by the general rule.

As regards the polar equation to the general involute,

$$r^2 = (G\phi)^2 + (G'\phi)^2,$$

it is obvious that, agreeable to the well-known case for the first involute,

$$\theta = \sin^{-1} \frac{dr}{ds} + \phi = \sin^{-1} \frac{G'}{r} + \phi^*,$$

where G' and ϕ are given by the solution of an algebraical equation of the $2i$ th degree, and which will therefore *usually* be incapable of expression in finite terms beyond the second involute. In the case of this involute the reducing equation is not a general biquadratic, but a form involving only square and no cube roots—being in fact reducible to a quadratic in ϕ^2 , as will at once be seen from the fact that we may write $(a\phi^2 + \gamma)^2 + 4a^2\phi^2 = r^2$.

Since $\frac{ds}{d\phi}$, *i. e.* $G + G''$, is a quantic of the degree i or ϕ , we

* ϕ and G' will form $2i$ systems of values. Will they be all applicable to the true involute, and how about the sign to be given to r ? It must, I think, be a matter of some delicacy and difficulty to answer these questions. For take even the first involute, where $\theta = \sin^{-1} \frac{a}{r} + \sqrt{\frac{r^2 - a^2}{a^2}}$,

we know, as a matter of fact, that if the first term is made to decrease as r increases, the positive sign of the square roots only must be employed, and of course the negative sign if the first term increases with r . Were we to reverse this rule, instead of the involute we should obtain what may be termed the counter-involute; *i. e.* a figure formed by points, each the *opposite* of every point in the involute in respect to its centre of curvature. Or, again, if a pair of parallel rulers were made always to touch a circle at opposite points, and the under parallel to *roll* round the circle, whilst each point in this line describes an involute, each point in the one above would describe a counter-involute. Or, again, if a string, by aid of a pin, were unwrapped *back* upon itself from a circle, the extremity would describe the extraneous curve. From this last observation it would seem as if the forced intrusion of a foreign curve into the polar equation of the involute resulted from the impossibility of affixing an absolute sign to the length of an arc—the condition of drawing a tangent always equal in length to the varying arc of a curve admitting of satisfaction without breach of continuity in two distinct modes.

learn that there may be i cusps to the i th involute, or any less number differing from i by an even integer. Also, since $\frac{ds^2}{ds} = G'$, the number of apses (in regard to the centre) may be any number inferior to and differing from i by an odd integer*. Also, since G represents the perpendicular on the tangent, the number of points where the tangent passes through the centre will follow the same law (although, of course, the two numbers need not be equal) as the number of the cusps; at such points the curve, after winding round the centre (it may be through one or more complete revolutions and a part of a revolution, or through only a part), will change its curve and wind round in the opposite direction; for it is clear that there can be no points of inflexion, since $\frac{ds}{d\phi}$ can never become infinite. These points last named may be termed points of retrocession. The cusps, of course, can only exist at points where the involute meets the parent curve.

Between any two cusps of an involute evidently must be comprised an odd number of the cusps of its parent curve; but, of course, not *vice versâ*; thus, *e. g.*, in the second involute, if there are no cusps, it will easily be seen that the curve possesses a simple loop enclosing the cusp of the first involute (its evolute), and consequently cutting the two branches of the latter, and so in general the disappearance of consecutive cusps in any involute will give rise to loops enclosing those cusps of the parent curve on the branches adjoining to which (on each side) cusps of the derived curve are wanting; (by a branch, I mean, of course, the portion of curve included between any two cusps, or between either of the two terminal cusps and infinity;) whether the absence of cusps of the involute on $2i$ consecutive branches of the parent curve implies the necessary existence of i distinct loops, one round every alternate one of the $2i-1$ cusps in which those branches meet, requires further consideration. It is clear that in an analytical sense the length of the arc of the parent curve included between any two cusps of the second curve must be taken as zero; the correct view (at least, for the purposes of this theory) being that the angle of convergence *continually* increases or decreases up to positive or negative infinity as we pass in one

* Thus we see that the apsidal distances from the centre are the arithmetical magnitudes of the roots of the equation formed by equating to zero the *discriminant* of $G+r=0$, which is of course of the degree $(i-1)$ in r .

If we consider the apses and cusps of any involute to form a combined group, an odd number of the points of this group will always be included between any two intersections of the curve with a circle concentric with the parent circle; for the limiting equation to $G^2 + G'^2 - r^2 = 0$ is $G'(G+G'')=0$. Every point in this combined group is a point of maximum or minimum elongation from the centre.

direction from point to point in a curve. Accordingly we ought not to say, as is usually done, that at a cusp the tangent is suddenly reversed in direction, but, rather, that the increment of the arc on passing through a cusp changes sign, as it ought to do according to first principles; for the *flow* of the incremental arc, from being concurrent with, becomes opposite to that of the rotating tangent line which carries it, or *vice versâ*. Thus in the common cycloid (a curve of infinite length to the eye and with an infinite number of cusps) we have $s = c \cos \phi$, which, subject to this interpretation, is perfectly true and self-consistent for the whole extent of the curve from infinity to infinity. In that case we have a visible representation of *quantity* undergoing an infinite number of periodic changes, although the *subject matter* of the quantity is continually changing and never recurs. In the case of the involutes of the circle, the number of those periodic changes is of course finite and equal to the number of the cusps. If A, B, C, D, . . . , L be the cusps in natural order on the curve whose involute is to be found, and if we call x the radius of curvature of the point of the involute corresponding to A (x being taken positive when this radius is in the position into which it would be brought by unwinding a string from the infinite branch adjacent to A), and if we form the series $x - AB + BC - CD \dots \pm KL$, where AB, BC, . . . are the arithmetical lengths of the branches, it is clear that at each term of this series in which a change of sign in the sum takes place the involute will have a cusp; if the number of branches is odd and x is negative, the sum may remain negative at whatever term we stop, and then there will be no cusp in the involute so engendered; but when the number of points A, B, C, . . . , L is even, then it is easy to see that one of the infinite branches must contain a cusp of the involute and the other be vacant. The second involute, whether cusped or not, manifestly consists of two parts symmetrically arranged about its apse. If we form a third involute by unwinding from this apse as origin, the figures so formed will again be symmetrical, and the cusps will lie at the vertices of an isosceles triangle; and now *every* involute of this symmetrical third involute will again be symmetrical, and so on continually, the number of conditions imposed on the parameters in order to ensure symmetry in the involute of the i th order being thus the integer part of $\frac{i-1}{2}$. When this

symmetry obtains, the algebraical equation requisite for determining the polar equation depends on the solution of an equation of only the i th instead of the $2i$ th degree; for it is clear that in this case the functions G^2 and $G^{1/2}$ may be made to contain only powers of ϕ^2 . Thus we may very easily write down the general polar equation to the absolutely general second involute, and

might, if it were worth while, do as much for the symmetrical class of third and fourth involutes, of which the former will contain two and the latter three arbitrary parameters, by solving a cubic and biquadratic equation in these two cases respectively.

It is easy to see also that the *arco-radial* equation to the symmetrical involute of an odd order is of only half the degrees in s and r^2 that it is of in the general case, and for the symmetrical involute of an even order, although of the same degrees in r^2 and s as in the general case, involves only the even powers of s .

A few words upon the second involute, and I have done; for it is difficult to deal with theory in any detail so as to be intelligible, or even safe, without the suggestive and regulative aid of drawn figures, which I have not yet been able to obtain in a form fit for use.

The two principal classes to distinguish in the second involute are the cusped and uncusped species. The cusped second involute winds round the parent curve upon which the extremities of its finite branch rests. The uncusped species crosses itself, and intersects each branch of the first involute of which it encloses the cusp, its node being on one side of it and its apse on the other. The transition case is when the unwinding begins from the cusp of the first involute; the second involute so obtained has a very singular point at that cusp, which may be regarded either as an abortive loop, or as a coincident pair of cusps*.

The general connecting equations for this involute may be put under the form

$$r^2 = \left(\frac{a}{2} \phi^2 + b \right)^2 + a^2 \phi^2,$$

$$\frac{ds}{d\phi} = \frac{a}{2} \phi^2 + (a+b),$$

$$\theta = \sin^{-1} \frac{a\phi}{r} + \phi,$$

where a is the radius of the circle; and there will be a loop or cusps according as $a+b$ is positive or negative; when $b = -a$,

* Mr. Crofton has noticed, in an ingenious paper published in the 'Mathematical Messenger,' that this involute is the locus of the centres of all the circles cutting orthogonally the originating circle and the parent first involute. This is seen very easily as follows.

$$p = a \left(\frac{\phi^2}{2} - 1 \right), \quad s' = p + p'' = a \frac{\phi^2}{2},$$

$$r^2 = p^2 + p''^2 = a^2 \frac{\phi^2}{4} + a, \text{ or } r^2 - a^2 = s'^2,$$

showing that the tangents to the circle and first involute from any point in the second are equal to one another.

we have the transition case, for which

$$r^2 = \frac{a^2}{4} \phi^4 + a^2, \quad s = \frac{a}{6} \phi^3,$$

and the arco-radial equation becomes

$$(r^2 - a^2)^3 = \frac{81}{4} a^2 s^4.$$

Another and more remarkable case occurs when $G^2 + G'^2$ becomes a perfect square, for then the degrees in s and r will sink to half their usual values: this occurs when $b = -\frac{a}{2}$, which is the case of a looped curve bisecting at its apse the radius drawn from the centre of the circle to the generating first involute.

We have in that case

$$G = \frac{a}{2} (\phi^2 - 1),$$

$$r = \frac{a}{2} (\phi^2 + 1) \text{ or } \phi = \sqrt{\frac{2r-a}{a}},$$

$$s = \frac{a}{2} \left(\frac{\phi^3 + 3\phi}{3} \right),$$

whence

$$9as^2 = (2r-a)(r+a)^2,$$

and

$$\theta = \sin^{-1} \sqrt{\frac{2ar-a^2}{r^2}} + \sqrt{\frac{2r-a}{a}} = \text{vers.}^{-1} \frac{a}{r} + \sqrt{\frac{2r-a}{a}},$$

a form even simpler than that of the first involute.

We may write this equation under the form

$$\theta = -2 \cos^{-1} \cdot \sqrt{\frac{\frac{1}{2}a}{r}} + \sqrt{\frac{r - \frac{1}{2}a}{\frac{1}{2}a}};$$

or, turning round the line from which θ is reckoned through a quarter of a revolution,

$$\theta = 2 \sin^{-1} \sqrt{\frac{\frac{1}{2}a}{r}} + \sqrt{\frac{r - \frac{1}{2}a}{\frac{1}{2}a}}.$$

Let now

$$\theta = 2\mathfrak{A}, \quad \frac{a}{2} \cdot r = \rho^2$$

(which is the same thing as if for x and y we substituted $x^2 - y^2$ and $2xy$), then

$$\mathfrak{A} = \sin^{-1} \cdot \frac{\frac{1}{2}a}{\rho} + \frac{\sqrt{\rho^2 - \left(\frac{a}{2}\right)^2}}{a}.$$

This is the polar equation to a known curve (of the kind used by Captain Moncrieff in his barbette gun-carriage). It is of the class of curves generated by a fixed point on a wheel rolling on a plane. Such a curve may be termed the *convolute* of a circle of a *pitch* denoted by the ratio of the distance of the fixed point *below* the centre to the radius of the revolving circle; thus a convolute of zero-pitch is the spiral of Archimedes, a convolute of unit pitch the first involute to the circle: the general equation to a convolute, when the distance below the centre is d and the radius a , is given by the Rev. James White in the last September Number of the Educational Series, and is easily shown to be

$$\vartheta = \sin^{-1} \frac{d}{\rho} + \frac{\sqrt{\rho^2 - d^2}}{a}.$$

Similarly, we may define the pitch of the second involute to be the ratio of the distance of its apse from the centre to the radius; and then we are conducted to the observation that whilst the convolute of full pitch is the first involute, the convolute of half pitch, on applying to it one of the simplest forms of M. Chasles's or Mr. Roberts's method of transformation (given in Dr. Salmon's 'Higher Plane Curves,' p. 236), viz. doubling the vectorial angle and squaring the radius vector, becomes converted into the second involute of half pitch. Since for this curve

$$r = \frac{a}{2} (\phi^2 + 1) = \frac{ds}{d\phi},$$

we see that it may be completely defined, without reference to any theory of involutes, as the curve whose radius of curvature at any point is equal to its radius vector reckoned from a given origin. It is the curve which completely satisfies the equation

$rd \cos^{-1} \frac{dr}{ds} = s$, the two arbitrary parameters which the complete integral of this equation should contain being furnished by the linear magnitude and angle of swing of the curve round the given origin*.

* This evolute possesses the property, which serves to characterize it completely, of cutting the originating *circle* (its second evolute) orthogonally. For when $r^2 = a^2$, $G^2 = 0$, *i. e.* the tangent to the curve passes through the centre. Moreover, since $G = 0$ gives $\phi = 1$, it follows that the curve cuts out of the circle an arc equal in length to the diameter. Summarizing such of its principal properties as have fallen in our way, we see that it bisects the line joining the centre of the originating circle and the cusp of the first involute; that it cuts the said circle orthogonally; that its radius

I conclude with the remark that if we regard the s and r^2 of the successive involutes as *rectilinear* coordinates to a variable point, the arco-radial equation will represent a peculiar class of unicursal algebraical curves. Thus the first involute will represent a pair (one for each branch of the curve) of coincident right lines, and the general second involute (taking r^2 and s^2 as the coordinates) a pair of coincident semicubical parabolas.

In making s vary continuously on passing a cusp, the corresponding abscissa from increasing must begin to decrease, or *vice versa*, according to the principles previously noticed.

Thinking of the recovery of the cusps and apses from the arco-radial equation, I have been led to consider a morphological property of a more general class of unicursal equations, which I think is likely to bear valuable fruit, and may possibly form the subject of another communication.

Athenæum Club,
September 1868.

of curvature is everywhere equal to its elongation from the centre; that it is a trajectory to a central force varying as the inverse cube of the shortest distance from the periphery of the originating circle; that its arco-radial equation is of only half the number of dimensions of the general involute of the same order; and that by the simplest form of quadratic transformation (viz. that which leaves unaltered the inclination of the tangent to the radius vector) it changes into the half-pitch circular convolute; not to add that its polar equation is even simpler than that of the first involute. Certainly, then, as it seems to me, it ought to take permanent rank among the spirals which have a specific name on the geometrical register: and for want of a better, with reference to the place where its properties first came into relief, it might be termed the *Norwich spiral*. Where it meets the first involute we have

$$\left(\frac{\phi^2+1}{2}\right)^2 = \frac{r^2}{a^2} = \phi^2 + 1,$$

or

$$(\phi^2+1)(\phi^2-3)=0;$$

so at the real intersections the radius vector is $2a$, and the perpendicular on the tangent, viz. $\left(\frac{\phi^2-1}{a} a\right)$, is a , showing that the tangent and radius vector at those points are inclined to each other at an angle of 30° .

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 232.]

May 28, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communication was read:—

“Observations of the Spectra of some of the Southern Nebulæ.” By Lieutenant John Herschel, R.E.

[Lieutenant Herschel, to whom the observations of the solar eclipse of August next have been entrusted by the Royal Society, has already employed the instruments, placed in his hands by the Royal Society for the observations of the eclipse, to good account by commencing an examination of some of the brightest of the nebulae of the southern heavens. The first results of this examination, which are contained in the present paper, were obtained at Bangalore, Madras Presidency, during the months of March and April 1868. The instruments consist of an equatorially mounted telescope of 5 inches aperture, driven by a clock furnished with a pendulum governor by Cooke and Sons, and a spectroscope by Messrs. Simms furnished with one prism of dense flint glass, and with a micrometer-screw and photographic scale for measuring the lines. The nebulae No. 4390 and No. 2102 have been described by me, *Phil. Trans.* 1864, p. 439, and *Phil. Trans.* 1866, p. 383. —W. H.]

No. 3531. [α 13^h 19^m: N.P.D. 136° 37': !!; globular cluster of stars; ω Centauri.]

March 25. A large cluster visible to the naked eye, oval-shaped, brighter towards the central part. *Spectrum an indefinable haze; no lines.*

No. 2197. [α 10^h 40^m: N.P.D. 148° 57': great nebula in Argus.]

Spectrum: *lines distinctly visible*, but not clear enough to be separated; approximate position $D+1.8 \pm .3$ ($D=2.30$, $E=3.68$, $b=3.97$, $F=5.03$). An unsatisfactory observation: to be looked for again.

No. 2017. [α 10^h 1^m: N.P.D. 129° 47': !!; planetary nebula, very bright, very large, little extended, *9M.]

March 31. Found with difficulty in the spectroscope. After a minute or two's examination the tube was accidentally disturbed; and before direction could be again obtained, clouds had gathered and work was stopped. Appearance in telescope a nebulous-looking star; under a higher power a nebulous-looking object with a much brighter nucleus or centre. In spectroscope *a continuous streak with a blotch of light nearly in the middle of its length two-fifths*, by estimation, from the red end; slit quite wide.

No. 2581. [α 11^h 44^m: N.P.D. 146° 27': a planetary nebula, small, round; blue=*7M.]

April 2. *A pretty well defined and bright short line* was distinctly visible in the spectroscope, accompanied by a considerably fainter and more refrangible companion. Principal line measured with the wires and found to be $D + 2.1 = 4.4$ ($b = 3.97$, $F = 5.03$), i. e. $b + 0.4$.

No. 4083. [$R\ 15^h\ 12^m$: N.P.D. $87^\circ\ 25'$: !!; globular cluster, very bright, large, extremely compressed in the middle.]

April 5. Seen in telescope as a slightly oval nebulous ball, easily seen but not very bright (perhaps owing to moon, nearly full); found with some difficulty in spectroscope; *a faint continuous spectrum* of considerable width; *no trace or suspicion of lines*.

No. 4173. (Seen easily in telescope; but looked for in spectroscope for two hours in vain.)

No. 4390. [$R\ 18^h\ 6^m$: N.P.D. $83^\circ\ 10'$: planetary nebula; very bright, very small, little hazy.]

April 6. Scarcely recognized as a nebula in the telescope. Seen in spectroscope: *a short bright line* with a fainter one on the more refrangible side, and a third strongly suspected. (Knowing so well the relative positions of the "usual" lines, it is impossible that an unprejudiced corroborative opinion can be offered on such slight foundation as I have.) A very slight extension laterally was given in this instance with the cylindrical lens.

No. 2102. [$R\ 10^h\ 18^m$: N.P.D. $107^\circ\ 59'$: !!; planetary nebula, very bright, little extended.]

April 9. Seen at once in telescope with low power; and seen distinctly in the spectroscope *as a bright and a faint line* (the third line not seen); principal line measured with the wires and found

$$= D + \left\{ \begin{array}{l} 2.14 \\ 2.16 \end{array} = 2.15 \right\}, \text{ or } b + 0.48, F = b + 1.06.$$

No. 1179. Nebula in Orion. Examined for comparison. The spectrum of this nebula *shows the three lines distinctly, and three only*; they were measured (with the wires), and the results were:—

$$D + \left\{ \begin{array}{l} 2.17 \\ 2.20 = 2.19 \\ 2.21 \end{array} \right\} = b + 0.52,$$

$$D + \left\{ \begin{array}{l} 2.31 \\ 2.40 = 2.36 \end{array} \right\} = b + 0.69,$$

and

$$D + 2.78 = b + 1.11.$$

The places and descriptions of the objects enclosed within brackets are taken from Sir John Herschel's "General Catalogue of Nebulæ" in the Phil. Trans. for 1864.

P.S.—The other day a storm passed over us. As there was a good deal of lightning, I took the opportunity to examine its spectrum. I saw, as I expected, numerous bright lines; the blue nitrogen one,

I suppose, much the brightest. A suspicion also of the red hydrogen-line C. I was much surprised at the brightness of the continuous spectrum, in which all the principal prismatic colours were brilliant.

June 11.—Lieut.-General Sabine, President, in the Chair.

The following communication was read:—

“On the Combustion of Hydrogen and Carbonic Oxide in Oxygen under great pressure.” By E. Frankland, F.R.S., Professor of Chemistry in the Royal Institution and in the Royal School of Mines.

In a former communication to the Royal Society* I described some researches on the effect of a diminution of pressure on some of the phenomena of combustion, and deduced therefrom the law that *the diminution in illuminating-power is directly proportional to the diminution in atmospheric pressure.*

Further experiments, made more than a year ago, on the nature of the luminous agent in a coal-gas flame†, led me to doubt the correctness of the commonly received theory first propounded by Sir Humphry Davy‡, that the light of a gas-flame and of luminous flames in general is due to the presence of solid particles. In reference to gas- and candle-flames, it is now well known that the fuliginous matter produced when a piece of wire-gauze is depressed upon such flames, and the sooty deposit which coats a piece of white porcelain placed in a similar position, are not pure carbon, but contain hydrogen, which is only completely got rid of by prolonged exposure to a white heat in an atmosphere of chlorine. On pursuing the subject further, I found that there are many flames possessing a high degree of luminosity which cannot possibly contain solid particles. Thus the flame of metallic arsenic burning in oxygen emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion (arsenious anhydride) at 218° C., whilst the temperature of incandescence of solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapour be made to burn in oxygen, or oxygen in carbonic disulphide vapour, an almost insupportably brilliant light is the result. Now fuliginous matter is never present in any part of this flame, and the boiling-point of sulphur (440° C.) is below the temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the more refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence.

Many other similar cases of the production of brilliant light from incandescent, gaseous, or vaporous matter might be cited; but I will

* Phil. Trans. vol. cli. p. 629 (1861).

† Lectures on Coal-gas, delivered at the Royal Institution in March 1867. Journal of Gas-lighting.

‡ Phil. Trans. for 1817, p. 75.

mention only one other. Amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now phosphoric anhydride, the product of this combustion, is volatile at a red heat; and it is therefore manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus-flame, which far transcends the melting-point of platinum. For these reasons, and for others stated in the lectures above quoted, I consider that incandescent particles of carbon are not the source of light in gas- and candle-flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapours. As a further generalization from the experiment above mentioned, I was led to the conclusion that dense gases and vapours become luminous at much lower temperatures than æriform fluids of comparatively low specific gravity, and that this result is to a great extent, if not altogether, independent of the nature of the gas or vapour, inasmuch as I found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air, but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

I have recently extended these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments were conducted in a strong iron vessel, furnished with a thick plate of glass of sufficient size to permit of the optical examination of the flame. The results are so remarkable that, although still far from being complete, I venture to communicate them to the Royal Society before the close of the Session. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure is too well known to need description. On increasing the pressure to two atmospheres, the previously feeble luminosity is very visibly augmented, whilst at ten atmospheres' pressure the light emitted by a jet about 1 inch long is amply sufficient to enable the observer to read a newspaper at a distance of 2 feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With a higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in air is well known to be continuous; burnt in oxygen under a pressure of fourteen atmospheres, the spectrum of the flame is very brilliant, and perfectly continuous.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density

of the gas; and this is in fact the case, for electric sparks passed as nearly as possible under similar conditions through hydrogen, oxygen, chlorine, and sulphurous anhydride emit light the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. When liquefied sulphurous anhydride is sealed up in a strong tube furnished with platinum wires, and the temperature then allowed to rise until the internal pressure amounts to three or four atmospheres, the passage of induction-sparks through the enclosed gas is attended with very brilliant flashes of light. Further, if a stream of induction-sparks be passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air be gradually augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks is observed, whilst on allowing the condensed air to escape, the same phenomena are observed in the reverse order.

The electric arc from fifty cells of Grove's battery is incomparably more brilliant when mercury vapour, instead of atmospheric air, is interposed in the path of the discharge between the carbon points. The gases and vapours just mentioned have the following relative densities :—

Hydrogen	1·0
Air	14·5
Oxygen	16·0
Sulphurous anhydride	32·0
Chlorine	35·5
Mercury	100·0

It is obvious that the above results have a very direct bearing upon the views now generally held regarding the constitution of the sun, stars, and nebulae; but I refrain from making any such application of them until I have the honour of laying before the Royal Society a complete account of these experiments.

June 18.—Lieut.-General Sabine, President, in the Chair.

The following communications were read :—

“Researches on Refraction-equivalents.” By J. H. Gladstone, Ph.D., F.R.S.

Since the paper of the Rev. T. Pelham Dale and myself “On the Refraction, Dispersion, and Sensitiveness of Liquids”*, our researches have been continued from time to time, and a good deal of attention has been paid to the subject in Germany. The permanence of the specific refractive energy of a body, notwithstanding change of temperature, aggregate condition, solution, or even chemical combination, has been confirmed, and upon this has been built the doctrine of Refraction-equivalents.

Our “specific refractive energy” is the refractive index of any substance minus unity, divided by the density—in symbolic language

* Philosophical Transactions, 1863, p. 317.

$\frac{\mu-1}{d}$. Professor Landolt's "Refraction-equivalent" is the same multiplied by the chemical equivalent, or $P \frac{\mu-1}{d}$.

The largest generalization arrived at is that the refraction-equivalent of a compound is the sum of the refraction-equivalents of its constituents. This has been sufficiently proved in a multitude of instances among the compounds of carbon, hydrogen, and oxygen, and it has been shown, or assumed to be the case, in the combinations of many other elementary bodies*.

My more recent researches have branched off into several lines; but an especial attempt has been made to answer the following questions. Have any of the elements more than one definite refraction-equivalent? and what are the refraction-equivalents of the metallic elements? A large mass of observations bearing on these points has been gathered together, and more or less collated; but it is yet imperfect, and my present object is rather to indicate the principal method of inquiry than to publish the actual results.

As the metals are opaque, their refractive indices cannot be determined in a direct manner as those of gaseous hydrogen, liquid phosphorus, crystallized carbon, and other transparent elements have been. An attempt must therefore be made to determine their effect on the rays of light by examining their compounds; but their crystalline salts are very frequently doubly refracting, owing to some peculiarities of internal structure, and, where they give only one spectrum, there are practical difficulties about the experiment that are not encountered in dealing with liquid bodies. The solutions of these salts have only one refraction; and it occurred to me that they might afford an easy means of determining the refraction-equivalents, first, of the compounds themselves, and, secondly, of their metallic and other constituents. In practice, many sources of error presented themselves, all of which tell upon the ultimate result, and which necessitated improved apparatus, and great care both in preparing the solutions and in taking the observations.

The method generally adopted was as follows:—An amount of salt representing the chemical equivalent was dissolved in n equivalents of water, and the refractive index and density of the solution were taken. From these was reckoned the refraction-equivalent; and subtracting from this n times the refraction-equivalent of water for the solar line A, there remained the refraction-equivalent of the dissolved salt for that part of the spectrum. That this fairly represents the action exerted on light by the chemical compound itself is supported by several considerations. 1st. In the few cases (such as chloride of sodium and sugar) where the refraction-equivalent of the

* See Brit. Association Report, 1863, Transactions of Sections, p. 12. Ibid. 1866, p. 37. Journal of the Chemical Society, ser. 2, vol. iii. p. 108. Landolt, Pogg. Annalen der Physik und Chemie, vols. cxvii., cxviii., and cxviii. Ketteler, Ueber die Farbenzerstreuung der Gase, 1865. Haagen, Pogg. Annalen, vol. cxviii. p. 125. Schrauf, Pogg. Annalen, vol. cxvii. p. 344. Wüllner, Pogg. Annalen, vol. cxxviii. p. 1.

substance has been obtained both in the solid and dissolved condition, it is found to be the same. 2nd. Solutions of several organic substances, such as formic and citric acids, give the theoretically correct equivalent for these substances. 3rd. The refraction-equivalent of a salt seems to be the same, whether it be dissolved in water or in alcohol. 4th. The refraction-equivalent of a salt in solution is not affected by altering the amount of water in which it is dissolved. 5th. The numbers reckoned for these salts in solution bear such a remarkable relation to one another as to force the conviction that they are made up of two components, the one depending on the metal, the other on the substance combined with it. To exhibit the nature and force of this argument, it would be necessary to tabulate a long series of results; but for the present I shall confine myself to the salts of potassium and sodium, with the corresponding hydrogen compounds.

Dissolved compound.	Common formula.	Refraction-equivalents.			Difference between potassium and sodium compounds.	Difference between potassium and hydrogen compounds.
		Potassium compound.	Sodium compound.	Hydrogen compound.		
Chloride.....	MCl	18.44	15.11	14.44	3.3	4.0
Bromide	MBr	25.34	21.70	20.63	3.6	4.7
Iodide	MI	35.33	31.59	31.17	3.7	4.2
Cyanide.....	MNC	17.12
Sulphocyanide	MSNC	33.40
Nitrate	MNO ₃	21.80	18.66	17.24	3.1	4.5
Metaphosphate	MPO ₃	19.48	18.68
Hydrate.....	MHO	12.82	9.21	5.95	3.6	6.8
Alcoholate.....	MC ₂ H ₅ O	27.68	24.28	20.89	3.4	6.8
Formiate	MCHO ₂	19.93	16.03	13.40	3.9	6.5
Acetate	MC ₂ H ₃ O ₂	27.65	24.05	21.20	3.6	6.5
Tartrate.....	M ₂ C ₄ H ₄ O ₆	57.60	50.39	45.18	3.6	6.2
Carbonate	M ₂ CO ₃	34.93	28.55	3.2
Sulphate	M ₂ SO ₄	30.55	26.20*	22.45	2.2	4.1
Bichromate	M ₂ Cr ₂ O ₇	79.9	72.9	3.5
Hypophosphite.....	M ₂ PH ₂ O ₂	26.94	20.93	3.0

From the above numbers several conclusions may be safely drawn. First, it is evident that the refraction-equivalents of the compounds of potassium differ very widely according to the nature of the electro-negative constituents; again, that the refraction-equivalents of the compounds of sodium differ *pari passu* with those of the potassium compounds, being always less by a number varying from 3.0 to 3.9. From this it may be fairly concluded that the electro-negative constituent has the same effect on light, whichever metal it is united with, and that the refraction-equivalent of potassium exceeds that of sodium by 3.4, or thereabouts. But does the Table afford the data for de-

* This number seems to be too high, but it is the mean of fairly accordant results. It is rejected in the calculation of average difference between potassium and sodium.

termining the absolute equivalent of one or other of these metals? It was at first thought that this would be arrived at by a comparison of the metal with hydrogen, the refraction-equivalent of which has hitherto been estimated at 1·3 (or 1·5 in the case of water); but the last column shows that the difference between potassium and hydrogen is not always the same, the differences being greater than can be attributed to errors of observation. Indeed the numbers seem to fall into two groups: with the mineral acids the differences lie between 4·0 and 4·7, while with water, alcohol, and the organic acids, they are always upwards of 6, varying indeed from 6·2 to 6·8, the average being 6·55. But it is in these last-mentioned compounds that the equivalent of hydrogen is believed to be 1·3. Assuming this, we may reckon the refraction-equivalent of potassium to be about $6·55 + 1·3$, that is, 7·85. We have, however, other means of arriving at an estimation. Chlorine, in such bodies as chloroform or tetrachloride of carbon, is represented by 9·8. Again, cyanogen, from the experiments of Dulong on the gas itself, may be taken at 9·2. Sulphur has a refraction of 16·0; hence sulphocyanogen may be reckoned as $16·0 + 9·2$, that is, 25·2. Subtracting these numbers from those of the respective potassium salts, we obtain the equivalent of the metal. Thus from different sources we may calculate for the value of potassium :—

From the chloride	8·6
„ cyanide	7·9
„ sulphocyanide	8·2
„ hydrate	8·3
„ alcoholate	8·1
„ formiate	7·8
„ acetate	7·7
„ tartrate	7·5

These numbers are tolerably close, though the equivalent of potassium deduced from its inorganic, would, on the whole, be higher than that deduced from its organic compounds. The mean of the first four computations is 8·2, that of the last four 7·8. Perhaps, pending further researches, it will be best to assume the mean of these numbers,

$$\text{Potassium} \dots\dots\dots = 8·0;$$

and since a sodium salt has a refraction-equivalent generally 3·4 lower than the corresponding potassium salt, we may reckon

$$\text{Sodium} \dots\dots\dots = 4·6*.$$

If, instead of taking the refraction-equivalent $P \frac{\mu-1}{d}$, we reckon

the specific refractive energy $\frac{\mu-1}{d}$, we obtain the following values:—

Potassium	0·205
Sodium	0·200

This implies that equal quantities of these two analogous metals

* Haagen, from the crystalline chloride, determined the refraction-equivalent of sodium for the hydrogen line d at 4·89.

exert very nearly, if not precisely, the same effect on the velocity of the rays of light.

Another deduction from the above Table is that already alluded to in regard to hydrogen; while in the organic acids it probably has the known refraction-equivalent 1·3, it would seem that in the others, viz. hydrochloric, hydrobromic, hydriodic, nitric, metaphosphoric, and sulphuric acids, it has a very much higher refraction-equivalent, one in fact which is little less than sodium, and falls short of potassium by only about 4·3. Hence we may deduce—

Hydrogen in organic compounds = 1·3

Hydrogen in mineral acids = 3·7

How far this conclusion may hold good throughout, and whether one number should be an exact multiple of the other number, must remain to be determined by future observations. It appears, however, to answer in the affirmative the question whether an element can have more than one definite refraction-equivalent.

Of course, from the Table given above, it would be easy to deduce values for each of the electro-negative constituents; but it would be safer to generalize from a larger number of instances.

The series of observations on potassium and sodium salts are the most complete and the most carefully revised of any which have yet been made; but if their refraction-equivalents are fixed, it becomes a much simpler matter to determine those of most other metals. Thus, of lead the nitrate and acetate have alone been examined; but as the refraction-equivalents reckoned for these salts differ from those of the corresponding potassium compounds in each instance by 4·1, the presumption is great that the refraction-equivalent of lead is very near $8\cdot0 + 4\cdot1$, or $12\cdot1$.

The following are numbers deduced from two or more salts of each metal. They must be looked upon only as approximately true; and any subsequent modification of the value $8\cdot0$ for potassium must lead to a corresponding modification of the whole series.

Metal.	Chemical equivalent.	Refraction-equivalent.	Specific refractive energy.
Potassium	39	8·0	0·205
Sodium	23	4·6	0·200
Lithium	7	3·9	0·557
Magnesium	12	3·7	0·308
Barium	68·5	7·8	0·114
Strontium	43·8	6·5	0·148
Calcium	20	5·2	0·260
Zinc	32·6	4·8	0·147
Nickel	29·5	5·1	0·173
Cobalt	29·4	5·2	0·177
Lead	103·5	12·1	0·117
Mercury	100	9·8	0·098
Ammonium	18	11·4	0·633

These numbers are suggestive in many ways; but I will only remark the very high refractive energy of lithium, the practical identity of nickel and cobalt, and the remarkable fact that the specific refractive energy of the metals are (with one or two exceptions) in the inverse order of their atomic weights.

“On a Method of making a Direct Comparison of Electrostatic with Electromagnetic Force; with a Note on the Electromagnetic Theory of Light.” By J. Clerk Maxwell, F.R.SS.L. & E.

The experiments described in this paper were made in the laboratory of Mr. Gassiot, who placed his great battery of 2600 cells of bichloride of mercury at the disposal of the author. Mr. Willoughby Smith lent his resistance-coils of 1,102,000 Ohms; Messrs. Forde and Fleeming Jenkin lent a sensitive galvanometer, a set of resistance-coils, a bridge, and a key for double simultaneous contacts; and Mr. C. Hockin undertook the observation of the galvanometer, the adjustment of the resistances, and the testing of the galvanometer, the resistance-coils, and the micrometer-screw. The electrical balance itself was made by Mr. Becker.

The experiments consisted in observing the equilibrium of two forces, one of which was the attraction between two disks, kept at a certain difference of potential, and the other was the repulsion between two circular coils, through which a certain current passed in opposite directions. For this purpose one of the disks, with one of the coils attached to its hinder surface, was suspended on one arm of a torsion-balance, while the other disk, with the other coil behind it, was placed at a certain distance, which was measured by a micrometer-screw. The suspended disk, which was smaller than the fixed disk, was adjusted so that in its position of equilibrium its surface was in the same plane with that of a “guard-ring,” as in Sir W. Thomson’s electrometers, and its position was observed by means of a microscope directed on a graduated glass scale attached to the disk. In this way its position could be adjusted to the thousandth of an inch, while a motion of much smaller extent was easily detected.

An exactly similar coil was placed at the other end of the torsion-balance, so as to get rid of the effects of terrestrial magnetism.

It was found that though the suspended disk and coil weighed about half a pound, a very slight want of equality between the opposing forces could be detected, and remedied by means of the micrometer.

The difference of potential between the disks was maintained by means of Mr. Gassiot’s great battery. To measure this difference of potential, it was made to produce a current through Mr. Willoughby Smith’s resistance-coil, and the primary coil of the galvanometer shunted with a variable resistance.

The current in the coils was maintained by a Grove’s battery, and was led through the secondary coil of the galvanometer.

One observer, by means of the micrometer-screw, altered the distance of the disks till the suspended disk was in equilibrium at zero. At the same time the other observer altered the shunt till the gal-

vanometer-needle was also in equilibrium. The micrometer-reading and the resistance of the shunt were then set down as the results of the experiment.

The mean of twelve satisfactory experiments, at distances varying from .25 to .5 inch, gave for the ratio of the electromagnetic to the electrostatic unit of electricity—

$$\begin{aligned} v &= 27.79 \text{ Ohms, or B. A. units.} \\ &= 277,900,000 \text{ metres per second.} \\ &= 174,800 \text{ statute miles per second.} \end{aligned}$$

This value is considerably lower than that found by MM. Weber and Kohlrausch by a different method, which was 310,740,000 metres per second. Its correctness depends on that of the B. A. unit of resistance, which, however, cannot be very far from the truth, as it agrees so well with Dr. Joule's thermal experiments.

It is also decidedly less than any estimate of the velocity of light, of which the lowest, that of M. Foucault, is 298,000,000 metres per second.

In a note to this paper the author gave his reasons, in as simple a form as he could, for believing that the ratio of the electrical units and the velocity of light, are one and the same physical quantity, pointing out the difference between his theory and those of MM. Riemann and Lorenz, which appear to lead to the same conclusion.

XLI. Intelligence and Miscellaneous Articles.

SECOND NOTE ON THE LAWS OF INDUCTION.

BY MM. JAMIN AND ROGER.

IN a previous communication we announced that the heat developed in the external circuit of a magneto-electrical machine, working with a constant velocity, was governed by the same law as in the case of an ordinary battery. Our machine consisted of six rotating plates, each provided with sixteen coils mounted in *tension*. The plates themselves were united in *quantity*; that is, the similar poles of each of them terminated in two common points, with which were connected the external circuit. Since then the machine has been arranged afresh, so as to combine the various plates in all possible manners. We will now give the results obtained by associating 1, 2, . . . 6, . . . n plates in quantity.

By joining in the same manner n piles, each having the electromotive force A and the resistance r , we have a single electromotor of the force A and resistance $\frac{r}{n}$; the intensity i of the current with an external resistance x will be

$$i = \frac{A}{\frac{r}{n} + x},$$

and the quantity of heat regenerated under these circumstances will be

$$C = \frac{A^2 r}{\left(\frac{r}{n} + x\right)^2}.$$

Now, working with 1, 2 ... 6, ... n plates, we have found that the machine verified this formula, taking for A^2 and r the values 813.12 and 100.

The following are some of the results :—

Resistance x .	Thermal units reproduced in the circuit.									
	Two plates.		Three plates.		Four plates.		Five plates.		Six plates.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
7.52	1.27	1.56	2.72	3.14	4.75	4.98	6.20	7.01	8.35	8.36
15.04	2.30	2.48	4.55	4.58	6.65	6.77	9.05	8.91	10.85	11.00
22.56	2.86	3.05	5.18	5.24	7.45	7.32	9.57	9.24	10.71	10.99
30.08	3.26	3.37	5.62	5.50	7.35	7.37	9.30	9.02	10.51	10.45
46.40	3.69	3.67	5.66	5.47	7.00	6.91	8.20	8.02	9.05	9.01
69.60	3.81	3.65	5.33	5.02	5.90	6.00	6.76	6.74	7.40	7.32
92.08	3.70	3.41	4.49	4.50	5.09	5.21	5.60	5.81	5.60	6.13

The laws we have established are not to be considered special to the instrument which we have used ; we consider that they apply to all electromotors of the same kind, and express the general laws of induction.

For the future it will be possible to calculate the work of these machines just like that of batteries, by which their use may be better regulated ; it will be necessary for this purpose to determine the constants A and r . We have obtained an insight into the nature of these constants, on which we will make a remark.

In the case of a constant battery, the internal resistance is that of the liquid in the troughs. This is not the case with our machine : r is merely a coefficient which satisfies the formula, it is equal to 110 turns of the rheostat ; and this resistance is only equal to 16.

Hence, for the very short and inverted currents which are developed in the plates when the induced currents pass, the bobbins have a very great resistance, much greater than that found with prolonged currents—than that, in short, which enters into Ohm's formula.

This circumstance alone characterizes induction, since it is the only change which is introduced into the formula ; but it alone is sufficient to explain the observed effects. If, in fact, the magneto-electrical machine had no other resistance than that of its wires, six plates together would represent more than six metres of normal copper wire ; it would act like a thermo-electric pile, and, having no resistance, would produce effects neither of light nor tension.

Tension, on the other hand, is the essential feature of induction ; it can only be produced by an electromotor of great resistance ; and by finding that this great resistance exists in the bobbins at the mo-

ment they become the seat of induction, we explain effects which are otherwise inexplicable, and show how dynamic electricity may be changed into electricity of tension.—*Comptes Rendus*, June 22, 1868.

THIRD NOTE ON THE LAWS OF INDUCTION.

BY MM. JAMIN AND ROGER.

If n' batteries, of the force A and the resistance r , are united in tension, they constitute a single one having the force $n'A$ and the resistance $n'r$; and, finally, if n of these mixed batteries were joined in parallel order, they would constitute a single one the constants of which would be $n'A$ and $\frac{n'r}{n}$. The heat reproduced in the external circuit x will be

$$C = \frac{n'^2 A^2 x}{\left(\frac{n'r}{n} + x\right)}.$$

We have connected our plates in couples of two, or in groups of three, or we have joined them all in tension; the results of experiment have confirmed the formula, as is proved by the following Table:—

Plates grouped.

Resistances.	One couple.		Two couples.		Three couples.		Two groups of three.		Six plates in tension.	
	Thermal units.		Thermal units.		Thermal units.		Thermal units.		Thermal units.	
	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.	Obs.	Calc.
58	2.25	2.44	6.80	6.67	10.52	10.93	7.27	8.05		
92	2.92	3.06	7.97	7.32	10.99	10.93	9.12	9.51		
144	3.35	3.51	7.43	7.25	9.80	9.99	11.05	10.98		
173	3.98	3.64	7.05	6.99	9.22	9.24	10.93	11.08		
239	3.86	3.69	6.27	6.37	7.90	7.97	10.35	10.88		
303	3.63	3.60	6.30	5.76	7.19	6.96	8.93	9.51		
452	3.38	3.25	4.59	4.65	5.26	5.33	7.40	8.63		
612	2.86	2.86	3.87	3.81	4.25	4.23	7.02	7.32	11.32	11.08

Our experiments may be thus generalized:—

Whenever bobbins in any number pass with a constant velocity at the same distances in front of magnets, they act like the elements of a constant battery. Notwithstanding interruptions or inversions of the current, the quantity of electricity produced is governed by Ohm's law, and the heat regenerated in the external circuit by that of Joule. These laws apply to all modes of grouping bobbins. The electromotive force of each bobbin varies with the velocity, and with all the circumstances of the construction.

The same is the case with the resistance ρ of this couple; it is always greater than the resistance of the wires composing the bobbin.

α and ρ had still to be measured—that is, compared with the constants of a known element. For this purpose we measured with twenty Bunsen's elements the heat produced in the external circuit r ;

it is given by the formula

$$C = \frac{(20a')^2 x}{(20\rho' + x)^2}.$$

By these experiments a' and ρ' could be measured. We had

For one bobbin	$a = 1.78$	$\rho = 6.87$
For a Bunsen	$a' = 0.753$	$\rho' = 1.00$
Ratio	$\frac{a}{a'} = 2.37$	$\frac{\rho}{\rho'} = 6.87$

With its 96 plates in tension our machine was equal as force to 225, and as resistance to 655 Bunsen's elements. In quantity it is only equal to 38 elements of the resistance 18. With other groupings we had the following results:—

	Value of the machine in Bunsen's elements.	
	Electromotive force.	Resistance.
Tension	226	655
Two groups of three	113	163.6
Three groups of two	75	72.3
Quantity	37.9	18.0

In all cases the effect of the machine and its arrangement might be so modified as to produce in a given position of the circuit the maximum calorific effect.—*Comptes Rendus*, July 1, 1868.

ON THE DIATHERMANCY OF CHLORIDE OF POTASSIUM.

BY G. MAGNUS*.

There are now found at Stassfurt large crystals of perfect transparency to which mineralogists have given the name *sylvine*. I have ascertained that sylvine has the same properties as rock-salt as regards its transparency for heat. A plate of sylvine 26 millims. in thickness allows 76 per cent. of the incident heat to traverse it, which is the same proportion as a plate of rock-salt of the same thickness from Stassfurt.

Melloni found that a plate of rock-salt of 2.6 millims. (that is, a tenth of the thickness of my plate) allowed 92.3 per cent. of the incident heat to traverse it; another plate, which he calls imperfectly transparent (*louche*), only allowed 62 per cent. to pass. Thus sylvine and Stassfurt rock-salt, with ten times the thickness, possess a diathermancy which is the mean of those which Melloni found for plates of rock-salt.

I had occasion to try a plate of rock-salt from Würtemberg, which gave 92.6 for a thickness of 26 millims.

The diathermancy of sylvine resembles that of rock-salt in another respect: it does not vary with the temperature of the source. I have found it the same for heat emitted by a lamp and for that arising from a vessel heated to 100°. Rock-salt has hitherto been the only substance known to be endowed with this property.—*Comptes Rendus*, June 29, 1868.

* Extract from a letter to M. Regnault.

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[FOURTH SERIES.]

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XLII. *Notes on the Chemical Geology of the Gold-fields of California.* By J. ARTHUR PHILLIPS*.

DURING three separate visits to the Pacific coast of North America I have had numerous opportunities of studying the geology of the Californian gold-fields, and of investigating the circumstances attending the chemical and physical changes which have formerly occurred and which, to a certain extent, are still taking place in those regions. Whilst carrying out these researches, various facts have come under my notice which appear to throw some light on the formation of auriferous veins, as well as on the distribution of the precious metal in the rocks in which it is found.

I have endeavoured to embody in the following paper some of the results of my inquiries, in the hope that, should the conclusions to which I have arrived be shown to be fallacious, the facts which have been collected may nevertheless assist other investigators in arriving at a correct interpretation of the phenomena attending the formation of auriferous veins. In doing this, I propose to first give a short description of the gold regions, and subsequently to treat of their quartz veins, alluvial deposits, hot springs, and salt lakes, all of which are intimately connected with the chemical geology of the districts in which they occur.

Rocks of the Gold-Region of California.

The great sedimentary metalliferous belt of California lies on the western slope of the Sierra Nevada, beginning in the neigh-

* Communicated by the Author, being the substance of a paper read before the Royal Society, March 12th, 1868.

bourhood of the Téjon Pass, and extending through the State to its northern limit. In consequence, however, of various local circumstances, different portions of this band are of very unequal importance as gold-producing districts.

The principal auriferous region may be said to occupy the western portions of the several counties of Mariposa, Tuolumne, Calaveras, Amador, El Dorado, Placer, Nevada, Sierra, and Plumas, with portions of the eastern sides of Yuba and Butte counties.

The apparently limited extent of the auriferous belt towards the north, in the counties of Plumas and Butte, is, according to the State Geological Survey, not owing to the thinning out of the gold-bearing formation in these localities, so much as to its being here, as well as in Shasta and Siskiyou counties, in a great measure covered by large masses of lava of very recent origin. This has been poured forth from Lassen's Butte and other volcanic cones in its vicinity; and, overflowing the older slates, it has covered them to a great depth with a non-metalliferous and almost indestructible capping.

Beyond Mariposa, in the southern portion of the gold-region, the slates are narrower and subject to interruption, and, from being more frequently and more extensively encroached on by the granite, they almost cease to form a continuous belt. This gradual decrease in the width of the auriferous formation from north to south, and the continuously increasing amount of metamorphism displayed, are very marked, since the granite progressively occupies a relatively larger portion of the Sierra, and by degrees descends lower down its flank.

The slates of the auriferous belt of California have been satisfactorily shown by Professor Whitney* to belong, for a great extent, to the Jurassic period, although the occurrence of numerous Triassic fossils in the gold-bearing rocks of Plumas county and elsewhere renders it more than probable that no inconsiderable portion of the slates in the heart of the gold-region are of that age. The sedimentary rocks of the great auriferous belt lying on the western slope of the Sierra Nevada principally consist of various slates and schists, sometimes containing nodules of white felspar, which is generally more or less decomposed. Among them are also found sandstones of various degrees of fineness (often transformed into quartzites), black talcose schists, with slates exhibiting a well-defined cleavage and silky structure, together with bands of crystalline limestone.

The rock constituting the principal mass of the Sierra Nevada is a granite containing only a small proportion of quartz, and in which but one species of felspar (oligoclase) is generally found.

* Geological Survey of California.

In addition to quartz, felspar, and mica, the latter of which is well crystallized and often of a brilliant black colour, this rock encloses titaniferous iron, and occasionally crystals of sphene of a light yellow colour. Magnetic oxide of iron is also present in notable quantities, and consequently imparts this property to the enclosing granite. The granite of the Sierra appears to retain its peculiar characteristics throughout a great portion of its extent; but towards the southern extremity of the chain hornblende begins to make its appearance as one of the constituents of the rock; the mica at the same time becoming less plentiful, and losing its brilliancy and dark colour.

Lying between the band of metamorphic slates and the great central granitic mass forming the more elevated portions of the chain, are found various eruptive rocks, such as syenites, diorites, and porphyries. The first of these pass into granites by almost imperceptible gradations, and appear to belong to the same geological period. The diorites, on the contrary, are of more recent origin, and contain neither sphene nor titaniferous iron, although they enclose a considerable amount of magnetic oxide of iron. In many places diorite is observed to traverse the granite in the form of distinct dykes. The porphyries are comparatively of small extent, and contain crystals showing the usual striæ, indicating the presence of plagioclastic felspars.

Quartz Veins.

All the crystalline rocks in the vicinity of the sedimentary deposits contain numerous veins of quartz, which traverse the granite and diorites near their point of junction with the slates, and, in addition to gold, enclose crystals of iron pyrites and other metallic sulphides.

These quartz veins of the crystalline rocks are comprised within a narrow zone running from south to north, along the western flank of the mountains above the great band of metamorphic slates forming the most productive portion of the gold-region, and extend, in the vicinity of the line of junction, nearly throughout its whole extent.

The quartz veins of the band of metamorphosed slates occupying the western slope of the Sierra, and lower on its flank than the crystalline rocks above described, are numerous and important. They are not, however, by any means equally distributed throughout the region of slates, but are chiefly concentrated in a belt having a width from east to west of some twelve or fifteen miles, and extending from south to north throughout the whole length of the formation. These veins, for the most part, follow the general direction of the strata in which they are enclosed. This parallelism, however, is not

absolute, since in many instances a vein, besides having a somewhat different direction from that of the bedding of the enclosing rock, throws off branches cutting the slate at considerable angles.

One of the most remarkable gold veins in California is that extending from Mount Ophir, in Mariposa county, to Mokelumne Hill in Calaveras, a distance of over seventy miles. This lead*, which frequently crops boldly out above the surface of the ground, and varies in thickness from six to sixty feet, may in some places be traced for many miles across the country, and often presents an outcrop like an immense white wall. Although by no means continuous, this may be considered as an axis with regard to the other veins of the region, which have generally an almost similar direction, and are most frequently grouped at no very considerable distances from it.

The gangue of the auriferous veins of California is invariably quartz, which is generally crystalline in its structure, or partially vitreous and semitransparent. In the majority of cases the quartz constituting an auriferous veinstone is ribboned in such a way as to have the appearance of a succession of layers parallel with the walls of the lead; and some one or more of these laminae are not unfrequently far more productive of gold than the others.

In some instances these parallel bands are separated from each other by a thin layer of quartz, slightly differing, either in colour or structure, from that forming the seams themselves; or they may be only distinguished by a difference of the colour or structure of two adjoining members of the series.

In many cases, however, laminae of the enclosing slates divide the vein into distinct bands; and in such instances it will be observed that the thickness of the interposed fragments of slate is sometimes not greater than that of a sheet of the thinnest paper. Cavities or druses containing crystals of quartz sometimes occur in all the auriferous veins of the country; and a certain amount of crystallization may also not unfrequently be remarked along the lines of junction of the several bands of which a vein is composed. In such cases crystallization appears to have been set up on the surface of the last-deposited stratum, which has induced the formation of a similar crop of crystals on the layer subsequently formed on its surface. Quartz crystals, however, rarely occur in notable quantities in any of the most productive veins; and when the structure of a lead is highly crystalline, and the quartz more than ordinarily transparent, it is con-

* In California a quartz vein is called a "lead," and in Australia a "reef." In the gold-regions of the latter country the term *lead* is applied to the deposits of the "deep placers."

sidered an unfavourable indication with regard to its auriferous character.

The quartz considered by miners as most "favourable for gold" is seamy, stained by oxide of iron arising from the decomposition of pyrites, mottled, and somewhat marble-like in appearance. In addition to ordinary quartz in a more or less crystalline form, amorphous hydrated silica, or semi-opal, and chalcedony are occasionally met with : and in some instances the opal is interfoliated between layers of true quartz, and is sufficiently auriferous to repay the expenses of treatment.

Generally the walls of auriferous veins are smooth and well defined, often affording evidence of a considerable amount of dynamic action ; and in the case of the lead being divided into bands by interfoliations of slate, these, to a less extent, are sometimes marked by groovings indicative of mechanical motion. Between the vein and the enclosing rock there is sometimes, but not always, a thin stratum of clay, which occasionally encloses small particles of gold.

The metallic minerals enclosed in the gangue of auriferous veins are ordinary iron pyrites, blende, and galena, and, less frequently, arsenical pyrites, magnetic and copper pyrites, and cinnabar. These sulphides invariably contain gold ; and veins in which some one or more of them does not occur in considerable amounts, are not regularly and lastingly productive. In the earlier days of quartz-mining these sulphides were allowed to escape, and the "free gold" was alone obtained ; but at the present time they are all carefully collected, and form an important addition to the profits of the miner.

Near the surface the iron pyrites and other sulphides become decomposed by the action of air and the percolation of meteoric water through the mass, staining the quartz of a red or brown colour, and leaving the gold in a form highly favourable for amalgamation. Under such circumstances numerous cubical moulds of iron pyrites are found in the veinstone ; and although this mineral has been entirely removed by chemical action, the cavities left contain finely divided gold, obviously liberated by the decomposition of pyrites.

Beneath the line of natural drainage of the country the sulphides remain undecomposed, and the extraction of gold becomes more difficult ; but if "rock" containing crystals of pyrites be placed in nitric acid and allowed to remain for a few hours in a warm place, the sulphide becomes dissolved and finely divided, or filiform gold will partially occupy the resulting cavities.

In addition to the gold thus enclosed in the metallic sulphides, grains and small plates of that metal are disseminated throughout the veinstone ; and this is particularly the case in the vici-

nity of certain coloured streaks, generally nearly parallel with the apparent lines of deposition of the quartz. When gold is found lining a cavity in a lode, or is enclosed in plastic clay, it is often in the form of crystals, which are usually either octahedrons or cubes modified by planes of the octahedron or rhombic dodecahedron. When crystallization takes place in a narrow fissure, the crystals are much flattened.

In one of the detrital beds in the vicinity of the village of Volcano, in the county of Amador, a distinctly marked quartz vein may be observed cutting through the gravel, and evidently formed by the action of water holding silica in solution. The mass of this lode consists of agate and chalcedony, portions of which are coloured by ferruginous stains. In speaking of this vein, Professor Whitney says, "This is not by any means an isolated case; other localities have been noticed where all the conditions necessary to the formation of quartz veins, similar in their general features to those in the auriferous slates, must have existed during the most recent geological epoch."

With reference to this subject, Dr. Oxlard* writes me as follows:—"The recent quartz vein containing argentiferous pyrites, of which I spoke to you when we met in California, is situated at Sulphur Springs, Bear Valley, about ten miles north-east of Borax Lake. This locality is resorted to by invalids for the purpose of bathing in the hot waters, which are supposed to possess medicinal properties, and which flow in very considerable quantities out of the foot of a hill on the north side of the creek.

"This water, which contains carbonate of soda, chloride of sodium, and a little borax, is perfectly bright and clear, but so hot, that it has to be conducted into a large reservoir to cool before it can be admitted into the tubs provided in the wooden bathing-sheds.

"The hill out of which this hot water flows rises about 400 feet above the cañon through which the waters make their escape, and near its summit a deposit of sulphur has been opened upon. This has been covered to a depth of several feet by vegetable mould, and Manzanita bushes are now growing luxuriantly upon it. In cutting a roadway from the valley to the sulphur deposit, a vein of friable quartz, evidently due to the action which has produced the solfatara, was cut through. This silica is banded in the direction of the vein, and contains finely divided iron pyrites. I took what I considered a fair sample of this quartz, which afforded me 16 ounces of silver per ton. Many of the nuggets of gold obtained in this immediate neighbourhood have been found with large pieces of cinnabar adhering to them."

* Formerly of the Toland Medical College, San Francisco, but now of Plymouth.

Some attention has also been recently directed to bands of auriferous slate found in the copper-bearing band west of the main gold-belt of the State, and in the foot hills of the Sierra. Of the deposits of this description, those near Lincoln, in Placer county, and at Quail Hill, in Calaveras, are the most remarkable.

In these localities the gold, instead of being obtained from well-defined veins chiefly composed of ordinary quartz, is enclosed in bands of siliceous slaty rock, extending north-west and south-east, and dipping in conformity with the other strata of the district. At the surface the Lincoln deposit is chiefly distinguished from the other slates of the neighbourhood by being, in places, deeply stained by oxide of iron, and forms a low outcrop of about 3000 feet in length and 200 feet in width.

Where this has been cut into, it has been found to consist of alternating bands of clay-slate stained by oxide of iron, and of a friable sandy quartz still retaining distinct evidences of stratification, and which under the microscope presents the appearance of a siliceous skeleton from which the more soluble constituents have been removed by aqueous agencies.

Through these bands, and running parallel with their stratification, dykes of felspathic rock make their appearance, and contain small opaque white crystals too much decomposed to admit of identification. These, like the slate itself, contain gold, but, in the case of the felspathic rock, in very small quantities only. In the slate are also found lenticular masses of blende and iron pyrites, both auriferous, with stains of carbonate of copper. The whole mass of the stratified rock contains a certain amount of "free gold," although the seams vary considerably in their yield, some of them affording mere traces, whilst others assay as much as 3 oz. per ton. The whole of the gold in the portions of the deposits as yet worked is in a "free" state; but it is probable that below the water-level it will be found associated with various sulphides, by the decomposition of which in the upper portions the precious metal has become liberated. The pyrites and blende, in addition to gold, afford by assay a small quantity of silver per ton; and particles both of native silver and native copper may be extracted by washing the decomposed slates.

As a general rule, the quartz lodes of California have not been found to be more subject to impoverishment in depth than other mineral veins, some of them having been worked on their dip for a distance of above 1200 feet without any diminution of their yield having taken place.

In order to determine the chemical and physical composition of quartz forming the gangues of some of the principal auri-

ferous veins of California, I have made analyses of the veinstones from several of the most productive mines, and have examined under the microscope thin sections from a great number of others. My experiments have, however, been chiefly confined to various lodes in the vicinity of Grass Valley, in the county of Nevada, with which I have had numerous opportunities of making myself acquainted, and among which are comprised some of the most productive veins of the country.

In preparing thin sections for examination I have carefully avoided the use of any kind of polishing-powder; and the arrangement made use of for examining them has generally been a $\frac{1}{4}$ objective, by Ross, with draw-tube and eyepiece magnifying about 400 linear. In some cases, however, a magnifying-power of above 1000 linear was employed.

Examination of Quartz from the Kate-Hayes Vein.—Fragments of quartz taken from near the surface at the Kate-Hayes vein, Grass Valley, and having a specific gravity of 2.59, gave the following results:—

Water lost at 212° F.	0.26	
Water lost by subsequent ignition	0.70	
Silica	93.32	
Alumina	2.03	
Iron	1.29	} Iron pyrites.
Sulphur	1.45	
Potash	0.40	
Soda	0.21	
Lime and magnesia	traces	
	<hr/>	
	99.66	

This vein is composed of parallel bands, some of which contain small quantities of chlorite and iron pyrites. The quartz of which it consists is of a greyish colour, not very distinctly crystallized, has a peculiar greasy appearance, and contains cavities, on the surfaces of which have been deposited guttate silica of a chalcedonious description. This rock contains gold in the form of large scales and plates.

When examined under the microscope with a low power, this quartz presents in some places a cloudy appearance, and is found to contain a few small cubical crystals of iron pyrites, together with cavities filled by chalcedonious matter. Under a higher power it is seen to contain fluid-cavities of about one-thousandth of an inch in diameter, and in which the ratio of the vacuity to the cubical content of the cavity is about 1 : 10.

This veinstone also encloses small cavities which appear to be lined with clay, and in which no vacuities can be seen. It likewise contains gas-cavities and *markings* very similar in appearance to the "glass-cavities" described by Mr. Sorby as occurring

in porphyritic pitchstone, in the leucite of the lava of Vesuvius, and in the augite of some trappean rocks. In the case of quartz from the auriferous veins of California, I am, however, inclined to believe that they are due to crystallization only, since I have never been enabled to discover globular vacuities in any of them, and precisely similar markings may be observed on many quartz crystals on examining their surfaces by the aid of an ordinary lens. Should a fresh deposit of silica immediately take place on the planes of a crystal thus marked, an appearance not unlike glass-cavities would be the result; and if the depressions on the original crystal should become coated with clay, by deposition from turbid water, before the formation of the succeeding stratum of quartz, a certain degree of opacity, like that observed in the quartz of many auriferous veins, would be caused. It is also obvious that crystallizations of other minerals might take place in such depressions; and these, becoming covered by the succeeding deposit of quartz, would have, to a great extent, the appearance of having been formed from a slag or glass in the act of cooling from a state of fusion.

That crystalline quartz is frequently produced by successive depositions we have abundant evidence from specimens in which a crystal is seen to enclose others, each successively smaller than that which immediately surrounds it. Clay and oxide of iron are also in this way deposited on the surfaces of crystals during the interval which elapses between the formation of two distinct layers, as may be observed at the tin mines of Schlackenwald and in other localities, where crystals are obtained of which the various layers are so separated from each other by a clayey deposit as to admit of being readily divided into a series of cup-like envelopes, finally enclosing a central nucleus.

Examination of Quartz taken from the Norambagua Mine.—Specimens of quartz taken from a depth of 500 feet at the Norambagua mine, situated about four miles south of Grass Valley, and having a specific gravity of 2.60, were selected for analysis and afforded the following results:—

Water lost at 212° F.	0.19
Water lost by subsequent ignition	0.39
Silica	96.29
Alumina	1.42
Iron	0.55
Sulphur	0.57
Arsenic	traces
Potash	0.36
Soda	traces
Lime and magnesia	traces
	<hr/> 99.77

The Norambagua vein is enclosed in a crystalline rock locally known as syenite; but in some parts of the hill its composition would rather entitle it to be classed as a diorite. This vein, which is seldom more than a foot in thickness, is divided by headings into thin laminæ parallel with its walls, and is chiefly composed of greasy-looking quartz, showing rather faint indications of crystallization. Between the several laminæ of silica there are not unfrequently thin deposits of blue slate, which have evidently been subjected to considerable pressure, and which cause the veinstone to be easily separated into parallel bands. In some places iron pyrites and arsenical pyrites are abundant; but in selecting specimens for analysis those portions of the veinstone containing a visible amount of sulphides &c. were avoided.

A microscopical examination of sections cut from this vein showed that it contains fluid-cavities of about $\frac{1}{5000}$ of an inch in diameter, in most of which vacuities could be observed. It also encloses a few gas-cavities and some apparently containing clay, together with markings like those observed in the quartz from the Kate Hayes.

Examination of Quartz from the North-Star Mine.—Some of the ordinary white quartz from the North-Star mine, near Grass Valley, having a specific gravity of 2.61, was next subjected to analysis, and gave the following results:—

Water lost at 212° F.	0.07
Water lost by subsequent ignition	0.03
Silica	98.61
Alumina	0.82
Peroxide of iron	traces
Potash	0.24
Soda	traces
	<hr/> 99.77

This vein, which sometimes thins down to a mere seam, and at others has a width of 6 feet, is one of the most productive in the district, and has been worked to a depth, on the inclination of the lode, of 750 feet. All the specimens were taken from near the bottom of the workings.

The quartz, of which it is principally composed, is of a milky colour, and presents indistinct crystalline faces in whatever direction it may be broken. Like nearly all the auriferous veins of the country, it exhibits a tendency to an arrangement according to bands parallel to the walls of the enclosing fissure, and, wherever it is most productive, contains iron pyrites and other sulphides.

Thin sections of this veinstone, examined under a high power, show a few minute fluid-cavities with vacuities, numerous small

cavities apparently containing clay, some larger ones in which the clay appears coloured by oxide of iron, and a great number of the markings before referred to, and which are believed to be due to crystallization.

The most remarkable circumstance in connexion with this vein is the occurrence within it of large quantities of hydrated silica of a bluish-grey colour: this has a resinous lustre, and possesses a distinct conchoidal fracture, but exhibits well-defined lines showing layers of deposition parallel with the other bands of which the lode is made up. It is found in the form of a series of lenticular deposits, often several fathoms in length, interlaminated with the ordinary crystalline quartz, from which it is separated by a thin heading of unctuous clay. This substance, which has the appearance of semi-opal, is frequently 6 or 8 inches in width in its thickest part, and gradually tapers off in all directions in such a way as to assume a lens-like form. This hydrated silica sometimes encloses iron pyrites, and contains a sufficient amount of the precious metal to render its treatment commercially advantageous.

Examination of Semi-opal from the North-Star Mine.—A specimen of this substance submitted to analysis had a specific gravity of 2·01, and afforded the following results:—

Water	8·08
Silica	86·69
Alumina	1·92
Protoxide of iron	0·99
Lime	1·44
Magnesia	0·23
Potash	0·26
Soda	0·05
	<hr/> 99·66

This substance, at a temperature of 212° F., lost water slowly during several days, and, placed under a bell-glass over fused chloride of calcium, continued to lose weight after 300 hours, when the loss amounted to 5·11 per cent. Under these circumstances no attempt was made to determine separately the water lost at 212° F.; but the amount of hygrometric moisture present must have been exceedingly small, as this specimen was taken at the same time and kept in the same cabinet with the crystallized variety, which lost only 0·07 per cent. of moisture at 212° F.

Examined by polarized light this mineral does not show any evidence of crystallization; but when a high power is employed, it appears full of small elliptical cavities in which no vacuities were observed.

Examination of undecomposed Auriferous Slate from Lincoln.—A specimen of the undecomposed auriferous slate from the

neighbourhood of Lincoln in Placer county, had a specific gravity of 2·55, and yielded the following results:—

Water lost at 212° F.	0·31
Water lost by subsequent ignition	1·88
Silica	72·05
Alumina	17·87
Peroxide of iron	1·98
Lime	1·65
Magnesia	0·38
Potash	1·71
Soda	2·40
	<hr/> 100·23

When examined under a low power, this slate is seen to be made up of angular fragments of quartz connected together by clayey matter, their largest planes being parallel to the lines of cleavage. It also contains numerous spots of peroxide of iron, which in many instances still retain the cubical form of iron pyrites. By the aid of a high power the quartz fragments are seen to enclose a few small gas-cavities; and one or two fluid-cavities, with distinctly visible vacuities, were observed.

Examination of decomposed Auriferous Slate from Lincoln.—The decomposed siliceous matter before referred to, somewhat resembling chalk in appearance, was found to have a specific gravity of 2·50, and on being analyzed gave the following results:—

Water lost at 212° F.	0·11
Water lost by subsequent ignition	traces
Silica	93·33
Alumina	4·46
Peroxide of iron	traces
Lime	0·75
Magnesia	0·43
Potash	0·52
Soda	traces
	<hr/> 99·60

When examined under the microscope, this substance appears to consist of the siliceous skeleton of the auriferous slate from which the aluminous and ferruginous matters have been almost entirely removed. The angular fragments of quartz, being thus deprived of the cementing material which originally bound them together and gave solidity to the mass, remain in a disintegrated and friable state, although still retaining distinct indications of stratification.

The number of well-defined fluid-cavities in the veinstones of the auriferous lodes of California is exceedingly limited; and in

order to obtain sections affording good examples, even of small size, it is necessary to select such bands as may be more than ordinarily crystalline, or to operate on thin fragments of crystals sometimes found lining the interior of drusy cavities.

In the more opaque and generally most auriferous portions of veins, the cavities are numerous but exceedingly small, and are often so opaque (apparently rendered so by being internally coated by a lining of clay) that no vacuities can be distinguished. Some of the bubbles, contained in cavities requiring an arrangement magnifying a thousand linear to be distinctly seen, were observed to move freely about.

Out of more than sixty sections of veinstone examined, only some six or eight were found to contain fluid-cavities of sufficient size to admit of any attempt at accurate measurement; but in all cases there appeared to be considerable differences in the relative dimensions of the vacuities and the enclosing cavities, and I consequently ascertained by direct experiment the temperatures at which they severally became filled. For this purpose a small bath, made of sheet brass with a piece of microscopic glass fitted into each side, was employed. The section to be examined was clipped on the inside of one of the small panes of thin glass, and the bath filled with spermaceti into which the bulb of a thermometer was inserted. By attaching this apparatus to the stand of a microscope, of which the body was fixed horizontally, heating the bath, and throwing the light of an oil-lamp on the object by means of a condenser, it was easy to study the appearance of the different cavities under various conditions of temperature.

The magnifying-power employed was in this case about 275 linear, and consequently only such sections as contained tolerably large cavities could be satisfactorily examined. In every instance, however, they were found to require very different degrees of temperature to become full, since in the same specimens some of the vacuities disappeared at 180° F., others filled at temperatures slightly above that of boiling water, whilst many, though much reduced in size, remained perfectly visible at 365° F.*

Alluvial Deposits.

Although a very large amount of the gold annually obtained was no doubt originally derived from auriferous veins, not more than about one-third of the precious metal now collected is procured directly from that source. The larger proportion of the gold brought into the market is derived from alluvial diggings,

* As an illustration of this fact, the results obtained by heating the section of a crystal of quartz from the great Mariposa vein may be given. Six several vacuities disappeared at the following temperatures, 250°, 260°, 280°, 290°, 310°, and 320° F. Others were not nearly full at 362° F.

in which it is associated with clay, sand, and gravel, from which it is separated by washing.

These gold-bearing drifts belong to at least two geological epochs, both comparatively modern, although the later period is distinctly separated from the earlier, its materials being chiefly derived from the disintegration and redistribution of the older deposits.

In California the more ancient deposits, or *deep placers*, are probably referable to a river-system different from that which now exists, flowing at a higher level or over a then less elevated continent, and frequently nearly at right angles to the direction of the main valleys of the present period.

The sources from which the supply of gold is derived are therefore the following:—

(1) Auriferous veins, most frequently enclosed in metamorphic slates.

(2) The distribution of gold by ancient river-systems, giving rise to *deep diggings*.

(3) The redistribution of placer gold by the present river-system, forming accumulations known as *shallow diggings*.

The deep placers are in many localities covered by a thick capping of lava; and in some places the eruptive matters covering the auriferous deposits occur in the form of basaltic columns, beneath which are found the layers of sand, gravel, and boulders with which gold is associated. The wood which occurs in these gravel-beds is either beautifully silicified, or is replaced by iron pyrites. In such localities it not unfrequently happens that a piece of wood will be observed of which one end had been converted into lignite, whilst the other remained unaltered; but the whole having subsequently become silicified, now presents the appearance of a combination of alabaster and jet, each portion still retaining the structure of the original wood.

In the more clayey strata of these sedimentary deposits, leaf-beds and impressions of leaves are not unfrequently found; and an examination of these made by Dr. Newberry authorizes the conclusion that the auriferous deposits lying beneath the lava are of tertiary age, and that in all probability they generally belong to the later Pliocene epoch*.

In many localities, and particularly between the south and middle forks of the Yuba River, these auriferous gravels have frequently, where exposed to denudation, a thickness of 120 feet, and of more than 250 feet where they have been protected by a volcanic capping. These vast auriferous beds are composed of rounded masses of all the eruptive and metamorphic rocks which occur above them in the Sierra. As a general rule, the lower

* Geological Survey of California, pp. 250 & 251.

portions consist of larger boulders than the upper; but this does not exclude the occasional appearance of large rounded masses of rock among the middle or upper members of the series.

Water-worn gold is, to a greater or less extent, disseminated throughout the whole mass of these deposits; not, however, with uniformity, but always with greater abundance near the bottom, and more particularly in direct contact with the *bed-rock*, which is invariably grooved and worn by the action of water. The materials of which these deep placers are composed are frequently consolidated into a sort of hard concrete by being firmly bound together by crystalline iron pyrites; and sometimes this cementing material partially consists of carbonate of lime and amorphous silica. In many cases, as at some places in the county of Nevada, the sand, gravel, and boulders are firmly held together by a material consisting almost entirely of silica, in such instances giving rise to an exceedingly hard conglomerate.

When in the deep placers this consolidation of the materials of which they are composed has taken place, it is known by the name of *cement*, and often necessitates the employment of large quantities of gunpowder for its removal.

The silica forming one of the ingredients of the auriferous conglomerates is rarely met with in a crystalline form; but near Kenebeck Hill I found a cavity, resulting from the junction of several pebbles, completely lined with well-defined crystals of quartz. One of these, on being sufficiently reduced in thickness by grinding, did not show under the microscope the usual fluid-cavities of the quartz of the ordinary veins of the country. In some localities the silicified wood is found in the form of large trees, of which the entire mass has become replaced by a siliceous deposit; whilst in others this transformation has taken place to a limited extent only, and on breaking up a large trunk the central portion still retains the structure and properties of the original wood.

Where the cementing material of the conglomerate chiefly consists of pyrites, the enclosed trunks of trees are usually replaced by that mineral, although, of two pieces of wood lying in close proximity to each other, one may have become silicified, whilst the other is replaced by iron pyrites. On examining under the microscope the leaves which often form thin beds in the sands and clays of the tertiary auriferous river-beds of the gold-regions, they are seen to be covered by small guttate deposits of semitranslucent silica; and on burning some leaves found in these drifts in the county of Nevada, I found that, after long digestion in hydrochloric acid, they afforded 25·21 per cent. of a siliceous ash.

A specimen of silicified wood (coniferous) from French Corral

lost in the water-bath 0·59 per cent. of moisture ; and by subsequent ignition the further loss amounted to 8·46 per cent., thus making the total loss 9·05 per cent. Another piece of wood from the same locality lost 0·56 per cent. at 212°, and 8·43 per cent. by subsequent ignition ; in this case, therefore, the total amount of water contained was 8·99 per cent.

The assay of several specimens of the cementing pyrites showed that it invariably contained a small but very variable amount of gold. In order to ascertain whether this exists in the form of water-worn grains mechanically enclosed within the sulphides, or in the form of spongy and filamentary particles similar to those met with in the pyrites of auriferous veins, I dissolved various samples in nitric acid, and afterwards subjected the residues to microscopical examination. In this way I detected granules of the precious metal which had evidently been worn by the action of water, whilst others appeared not to have been subjected to such attrition. However, on examining pyrites forming the substance of various fossilized trees, I was unable, when the samples were taken from the inside of the trunks in order to avoid the possibility of accidentally attached particles, to detect an appreciable amount of gold, even when the assays were made on several hundred grains of the material ; but my experiments were conducted on specimens from one locality only (French Corral), and in Australia very different results appear to have been obtained. Mr. Ulrich states that "in the gold-drifts pyrites is often found incrusting or entirely replacing roots and driftwood ; such specimens very quickly decompose on exposure to the atmosphere, and samples have, on assay by Messrs. Daintree, Latta, and Newberry, yielded from a few pennyweights to several ounces of gold per ton." According to Mr. H. A. Thompson, "a beautiful specimen of crystallized iron pyrites, deposited on a piece of wood taken from the drift immediately below the basalt at Ballarat, gave, by assay, 40 oz. of gold per ton ; and in another case, where only the pyrites from the centre of an old tree-trunk was examined, the yield was over 30 dwt. of gold per ton. Some of the fine dust obtained in washing out the gold at the Royal Saxon claim, Ballarat, yielded, by assay, over 15 oz. of gold per ton. When placed under the microscope, this dust was seen to be composed of minute crystals of pyrites aggregated into round pellets from $\frac{1}{300}$ to $\frac{1}{100}$ of an inch in diameter, the surfaces being roughened by the projecting angles of the crystals, and unwater-worn"*.

[To be continued.]

* Notes on the Physical Geography, Geology, and Mineralogy of Victoria. By Alfred R. C. Selwyn and George H. F. Ulrich. Pp. 36. (Melbourne, 1866.)

XLIII. *On Discontinuous Movements of Fluids.*

By Professor HELMHOLTZ*.

IT is well known that the hydrodynamic equations give exactly the same partial differential equation for the interior of an incompressible fluid free from friction, and the particles of which do not rotate, as obtains for stationary currents of electricity or heat in conductors of uniform conductivity. It might be expected, therefore, that, for the same shape of the space traversed and for like conditions of boundary, the form of the stream for fluids would be the same as that for electricity and heat, with the exception of trifling differences caused by secondary conditions. In fact, however, the differences in many cases are very evident and profound between the arrangement of the current of a fluid and that of the above-mentioned imponderables.

Such differences are especially obvious if the current enters through an opening with sharp edges, into a wider space. In such cases the radiant lines of electricity separate from one another in all directions immediately on passing the opening; while a fluid current, whether water or air, moves forwards at first from the orifice in a compact stream, which is then usually dissipated in gyrations at a greater or less distance. The parts of the fluid of the larger vessel which lie in the neighbourhood of the orifice, but at the sides of the current, may, on the contrary, remain completely at rest. Every one is acquainted with this sort of motion, which is shown clearly by a current of air impregnated with smoke. In fact in these phenomena the compressibility of the air is of little influence; and air shows, with very few deviations, the same forms of motion as water.

Such great differences between what actually takes place and the deductions from the theoretical analysis hitherto accepted, must cause physicists to regard the hydrodynamical equations as a practically very imperfect approximation to the truth. The cause of this discrepancy might be supposed to lie in the internal friction of the fluid, although the divers strange and saltatory irregularities which every one has encountered who has experimented upon the motions of fluids can in no wise be accounted for by the continuous and uniform action of friction.

The investigation of the case where periodical motions result from a continuous air-current, as, for instance, in organ-pipes, convinced me that such an action could only arise by a discontinuous motion of the air, or at least by an approximately discontinuous one. Hence I was led to the discovery of a condi-

* Translated by Frederick Guthrie, F.R.S.E., from the *Monatsbericht der königlich preussischen Akademie der Wissenschaften zu Berlin*. April 1868, p. 215.

tion which must be taken into account in the integration of the hydrodynamic equations, and which has, as far as I am aware, been hitherto neglected. By taking this condition into account, on the other hand, where the calculation can be carried out, forms of motion are obtained such as are observed in reality. The circumstance in point is the following:—

In the hydrodynamic equations the velocity and the pressure of the current-particles are treated as continuous functions of the coordinates. But, on the other hand, if we consider a perfect liquid (that is, one not subject to friction), there is nothing essential in its nature to prevent two immediately neighbouring liquid layers from slipping past one another with finite velocity. At all events, those properties of fluids which are considered in the hydrodynamic equations, namely the constancy of the mass in all units of volume and the equality of the pressure in all directions, clearly offer no obstacle to the occurrence of tangential motions of finite difference of magnitude on both sides of a plane passing through the interior. The components perpendicular to the surface of the velocity and the pressure must, on the other hand, of course be equal on both sides of such a surface. In my work on gyratory movements*, I have already pointed out that such a case must occur if two previously separated masses of water come into superficial contact when they have different motions. In my above-mentioned work I was led to the idea of such a *surface of separation*, or, as it was then called, "*gyration-surface*," by the conception of lines of gyration arranged continuously along a surface, the mass of which may be vanishingly small, while its moment of rotation remains finite.

Now, in a fluid which at first is at rest, or in continuous motion, a finite difference of motion between immediately neighbouring particles can only be brought about by a moving force which acts discontinuously. Of external forces, we here only consider the force of impact.

But there exists also in the interior of the fluids a cause which may give rise to discontinuity in the motion. Pressure, indeed, may assume any positive value, and the density of the fluid will vary continuously with the pressure. But the moment the pressure passes zero and commences to become negative, a discontinuous change in the density takes place, the fluid is broken asunder.

Now the magnitude of the pressure in a moving fluid depends upon the velocity, and, indeed, in incompressible fluids the diminution of the pressure under otherwise similar circumstances is directly proportional to the *vis viva* of the moving particles. If the *vis viva* exceeds a certain amount, the pressure must in fact

* *Journal für reine und angewandte Mathematik*, vol. lx.

become negative, and the fluid must be torn apart. At such a place the accelerating force which is proportional to the differential coefficient of the pressure becomes, of course, discontinuous, and consequently the condition is fulfilled which is necessary to bring about a discontinuous motion in the fluid. The motion of the fluid in passing by such a place must be of such a nature that a surface of separation is there formed.

The velocity necessary to bring about the rupture of the fluid is that which the fluid would assume if it were to flow into *vacuum* under the pressure which the fluid would have at the same place when at rest. This is certainly a comparatively considerable velocity; but we must bear in mind that if fluids are to flow continuously like electricity, the velocity at every sharp edge around which the current turns must be infinitely great*. It consequently follows that *every perfect geometrically sharp edge by which a fluid flows must tear it asunder and establish a surface of separation, however slowly the rest of the liquid may move*. When the edges are imperfectly formed, and rounded, such a separation requires a certain increased velocity. Pointed projections on the side of the orifice must act in the same manner.

In regard to gases, the same condition obtains with them as with liquids, with the exception that the *vis viva* of the motion of a particle is not directly proportional to the diminution of the pressure p , but to the quantity p^m , where, taking into account the cooling of the air by expansion, $m = 1 - \frac{1}{\gamma}$, γ being the ratio between the specific heat with constant pressure and that with constant volume. For atmospheric air, the exponent m has the value 0.291. Since this is positive and real, p^m and p can only decrease to nothing, and not become negative as the velocity becomes high. It would be otherwise if gases simply followed Mariotte's law, and did not suffer any change of temperature. The value $\log p$ would then enter instead of p^m ; and this value may become infinite and negative without p becoming negative. Under this condition, a tearing asunder of the mass of air would not be necessary.

We may satisfy ourselves of the actual existence of such discontinuities by allowing a stream of air impregnated with smoke to flow through a round hole or cylindrical tube with moderate velocity so that no hissing ensues. Under favourable circumstances thin streams may in this way be obtained of a line in diameter and several feet long. Within the cylindrical

* At the very small distance ρ from a sharp edge the faces of which are inclined at an angle α , the velocity becomes infinite with ρ^{-m} , where

$$m = \frac{\pi - \alpha}{2\pi - \alpha}.$$

surface the air is then in motion with constant velocity; but outside the stream, even quite close to it, the air is not at all, or scarcely at all, moved. This sharp separation may also be seen very distinctly if a calmly flowing cylindrical current of air be led through the apex of a flame. A sharply bordered piece of the flame is cut out, while the rest of it remains quite undisturbed, or at most a very thin sheet, corresponding to the boundary layers caused by the friction, is carried along a little way with the air-current.

With regard to the mathematical theory of these motions, I have already given the limiting conditions for an internal surface of separation of the liquid. These conditions are, that the pressure on both sides of the surface must be the same, as also must be the components of the velocity in a direction perpendicular to the surface of separation. Since, now, the motion throughout the interior of an incompressible fluid, the particles of which have no rotary motion, is fully known when the motion of its entire surface and its interior discontinuities are given, it follows generally that when a fluid is rigidly enclosed, we have only to consider the motion of the surface of separation and the changes in the discontinuity.

Such a surface of separation can be treated mathematically exactly as if it were a surface of gyration—that is, as if it were completely covered with lines of gyration (*Wirbelfäden*) of infinitely small mass, but finite moments of rotation. In every surface unit of such a surface of gyration there is one direction in which the components of the tangential velocities are equal. This gives at once the direction of the lines of gyration at the corresponding place. The moment of these lines must be made proportional to the difference shown by the perpendicular components of the tangential velocity on both sides of the surface.

The existence of such lines of gyration for an ideal fluid without friction is a mathematical fiction which facilitates the integration. In an actual liquid subjected to friction this fiction is quickly realized, because the bordering particles are set in rotation by the friction, and then immediately are formed lines of gyration of finite gradually increasing mass, while the discontinuity of the motion is thereby at the same time equalized.

The motion of a surface of gyration, and of the lines of gyration lying in it, are to be determined according to the rules established in my work on gyratory motion. The mathematical difficulties of this problem can only, it is true, be overcome in a few of the more simple cases. But in many other cases we may at least draw conclusions concerning the direction of the changes by means of the method of viewing the matter just described.

It is especially to be remarked that, according to the laws

established for gyratory motions, the lines, and with them the surfaces of gyration, can neither originate nor disappear in the interior of a fluid free from friction, but that, on the contrary, every line of gyration must retain the same moment of rotation constant. It is further clear that the lines of gyration upon a gyration surface must advance with a velocity which is the mean of the velocities existing on both sides of the surface. It hence follows that a surface of separation can only be elongated in that direction in which the stronger of the two contiguous currents in it moves.

I sought, in the first place, for examples of persistent surfaces of separation in stationary currents which would allow of integration, in order to test whether the theory gives forms of currents which correspond better with experiment than when the discontinuity of the motion is neglected. If a surface of separation which divides flowing from still water has to remain stationary, the pressure along the surface must be the same in the moving layer as in the stationary one; whence it follows that the tangential velocity of the particles of water throughout the whole extent of the surface must be constant. So also must be the density of the fictitious lines of gyration. The beginning and end of such a surface can only lie at the side of the vessel, or at an infinite distance. In the first case they must be tangential to the side of the vessel, provided that the latter be continuously bent, because the component of the velocity perpendicular to the side of the vessel must be equal to nothing.

The stationary forms of the surfaces of division are distinguished, as experiment and theory alike indicate, by a remarkably high degree of alterability when subjected to the least disturbance, so that they comport themselves in some degree like bodies in unstable equilibrium. The remarkable sensitiveness to sound of a cylindrical current of air impregnated with smoke has already been described by Dr. Tyndall. I have verified Dr. Tyndall's experiments. This is clearly a property of the surfaces of separation, which is of the greatest importance in sounding musical pipes.

Theory points out that, wherever an irregularity is formed on the surface of an otherwise stationary current, this must give rise to a progressive spiral unrolling of the corresponding part of the surface; the corresponding portion, moreover, advances along the current. This endeavour towards spiral unrolling at every interruption is also easily recognizable on observing the currents. According to theory, a prismatic or cylindrical current could be infinitely long. But in practice such a current cannot be formed, because in an element which is so easily moved as air it is impossible entirely to avoid small disturbances.

It is easy to see that the conditions of the state of rest are satisfied by such an infinitely long cylindrical current which, issuing out of a tube of the same diameter, enters into a still external fluid, and contains throughout fluid which moves parallel to its axis with uniform velocity.

I shall here merely give a sketch of the mathematical treatment of a case of the inverse kind, where the current passes out of a wide space into a narrow channel, in order at the same time to give an example of a method by which certain problems can be solved in the doctrine of potential functions which have hitherto presented difficulty.

I confine myself to the case where the motion is "stationary," and depends alone upon two rectangular coordinates x, y , and where, further, in the frictionless fluid there are no rotating particles, and consequently none such can arise. For the particle of fluid at the point (x, y) , let us denote by u the component velocity parallel to x , and by v that parallel to y . Then two functions of x and y may, as is well known, be found such that

$$\left. \begin{aligned} u &= \frac{d\phi}{dx} = \frac{d\psi}{dy}, \\ v &= \frac{d\phi}{dy} = -\frac{d\psi}{dx}. \end{aligned} \right\} \dots \dots (1)$$

By means of these equations the condition is at once fulfilled in the interior of the fluid, that the mass remains constant for every unit of volume, namely

$$\frac{du}{dx} + \frac{dv}{dy} = \frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} = \frac{d^2\psi}{dx^2} + \frac{d^2\psi}{dy^2} = 0. \dots (1a)$$

With the constant density h , and when the power of the external force is represented by V , the pressure in the interior of the fluid is given by the equation

$$\left. \begin{aligned} V - \frac{p}{h} + C &= \frac{1}{2} \left[\left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 \right] \\ &= \frac{1}{2} \left[\left(\frac{d\psi}{dx} \right)^2 + \left(\frac{d\psi}{dy} \right)^2 \right] \end{aligned} \right\} \dots \dots (1b)$$

The curves

$$\psi = \text{const.}$$

are the current lines of the fluid, and the curves

$$\phi = \text{const.}$$

are orthogonal to them. The latter are the curves of equal power in the case of electricity, or of equal temperature in the case of heat, flowing in a stationary current in conductors of constant conductivity.

From equation (1) we get as integral equation that the value $\phi + \psi i$ is a function of $x + yi$ (where $i = \sqrt{-1}$). The solutions hitherto found generally express ϕ and ψ as a sum of terms which are themselves functions of x and y . But, inversely, we may regard $x + yi$ as a function of $\phi + \psi i$ and develope. In problems dealing with currents between two rigid walls, ψ is constant along the borders; so that if ϕ and ψ be laid down as rectangular coordinates in a plane, we have, in a strip of this plane bounded by two parallel straight lines $\psi = c_0$ and $\psi = c_1$, to seek the function $x = yi$ so that it may correspond at the edge to the equation of the wall, and in the interior assume the given discontinuities.

A case of this kind is when we put

$$x + yi = \Lambda \{ \phi + \psi i + e^{\psi + \psi i} \}, \quad . \quad . \quad . \quad (2)$$

or

$$x = \Lambda \phi + \Lambda e^{\psi} \cos \psi,$$

$$y = \Lambda \psi + \Lambda e \sin \psi :$$

for the value $\psi = \pm \pi$, y is constant, and

$$x = \Lambda \phi - \Lambda e^{\psi}.$$

If ϕ varies from $-\infty$ to $+\infty$, x changes at the same time from $-\infty$ to $-\Lambda$, and then back again to $-\infty$. The current-curves $\psi = \pm \pi$ correspond, therefore, to the current along two straight walls, for which $y = \pm \Lambda \pi$, and x varies between $-\infty$ and $-\Lambda$.

If, therefore, we use ψ to express the current-curves, equation (2) corresponds to a current which flows into infinite space from a canal bordered by two parallel planes. At the edge of the canal, however, where $x = -\Lambda$ and $y = \pm \Lambda \pi$, and, further, where $\phi = 0$ and $\psi = \pm \pi$,

$$\left(\frac{dx}{d\phi} \right)^2 + \left(\frac{dy}{d\phi} \right)^2 = 0,$$

$$\left(\frac{d\phi}{dx} \right)^2 + \left(\frac{d\phi}{dy} \right)^2 = \infty.$$

Electricity and heat may flow in this manner; liquids would be torn asunder.

If from the edge of the canal there should proceed stationary lines of division, which of course would be continuations of the current-lines $\psi = \pm \pi$ passing along the wall, and if exteriorly to these lines of division which border the current fluid there should be rest, then the pressure must be the same on both sides of the lines of division. That is, along those portions of the

lines $\psi = \pm \pi$ which correspond to the independent divisional lines we must have, according to (1b),

$$\left(\frac{d\phi}{dx}\right)^2 + \left(\frac{d\phi}{dy}\right)^2 = \text{const.} \quad . \quad . \quad . \quad (3)$$

In order now to maintain the chief features of the motion given in equation (2), let us add to the above expression for $x + yi$ another term $\sigma + \tau i$, which is also a function of $\phi + \psi i$.

We have then

$$\left. \begin{aligned} x &= A\phi + Ae^{\phi} \cos \psi + \sigma, \\ y &= A\psi + Ae^{\phi} \sin \psi + \tau; \end{aligned} \right\} \quad . \quad . \quad . \quad (3a)$$

and we must determine $\sigma + \tau i$ so that, along the free portion of the divisional surface $\psi = \pm \pi$, we may have

$$\left(A - Ae^{\phi} + \frac{d\tau}{d\phi}\right)^2 + \left(\frac{d\tau}{d\phi}\right)^2 = \text{const.}$$

This condition is fulfilled if, in the same place, we make

$$\frac{d\sigma}{d\phi} = 0, \text{ or } \sigma = \text{const.}, \quad . \quad . \quad . \quad (3b)$$

and

$$\frac{d\tau}{d\phi} = \pm A \sqrt{2e^{\phi} - e^{2\phi}}. \quad . \quad . \quad . \quad (3c)$$

Since ψ is constant along the wall, we can integrate the last equation in regard to ϕ , and convert the integral into a function of $\phi + \psi i$ by putting $\phi + i(\psi + \pi)$ everywhere in place of ϕ . We thus, after properly determining the constants of integration, get

$$\sigma + \tau i = Ai \left\{ \sqrt{-2e^{\phi + \psi i} - e^{2\phi + 2\psi i}} + 2 \arcsin \left[\frac{i}{\sqrt{2}} e^{\frac{1}{2}(\phi + \psi i)} \right] \right\}. \quad (3d)$$

The points of divergence of this expression are where $e^{\phi + \psi i} = -2$, that is, where $\psi = \pm (2n + 1)\pi$ and $\phi = \log 2$; so that none of them lie in the interval from $\psi = +\pi$ to $\psi = -\pi$. The function $\sigma + \tau i$ is here continuous.

Along the wall

$$\sigma + \tau i = \pm Ai \left\{ \sqrt{2e^{\phi} - e^{2\phi}} - 2 \arcsin \left[\frac{1}{\sqrt{2}} e^{\frac{1}{2}\phi} \right] \right\}.$$

If $\psi < \log 2$ the whole value is purely imaginary, so that $\sigma = 0$, while $\frac{d\tau}{d\phi}$ assumes the value given in (3c). This portion of the lines $\psi = \pm \pi$, therefore, corresponds to the free portion of the stream.

If $\phi > \log 2$, the whole expression as far as $+A\pi$ becomes real, which latter belongs to the value of τi through its relation to $y i$.

The equations (3a) and (3d) correspond, therefore, to a pouring out of a boundless trough into a canal bordered by two planes, the breadth of the canal being $4A\pi$, and the length of its walls being from $x = -\infty$ to $x = -A(2 - \log 2)$. The free divisional line of the current liquid is at first bent from the edge of the orifice a little towards the side of the positive value of x , where for $\phi = 0$, $x = -A$, and $y = \pm A(\frac{3}{2}\pi + 1)$ it attains its greatest x -value. It then enters the interior of the canal, and at last approaches the two lines $y = \pm A\pi$ asymptotically; so that, finally, the breadth of the stream as it flows out is only equal to half the breadth of the canal.

The velocity along the divisional surface and at the straight end of the stream flowing out is $\frac{1}{A}$. Along the rigid wall, and in the interior of the liquid, the velocity is always less than $\frac{1}{A}$, so that this form of motion may take place with all values of velocity of efflux.

One especially remarks in these examples how it is shown that the form of the liquid current in a tube may be determined for a long way by the shape of the first portion.

Addition, relative to electrical dispersion.—If in equation (2) we look upon the value ψ as the power of the electricity, we get the distribution of the electricity in the neighbourhood of the edge of two plane and almost touching screens under the condition that the distance between the two may be considered vanishingly small compared with the radius of curvature of their curved edges. This is a very simple solution of the problem which Clausius* has discussed. The same distribution of electricity is obtained as was found by Clausius, as far, at least, as it is independent of the curvature of the edge.

I may add that the same method suffices to determine the distribution of the electricity on two parallel infinitely long plane strips whose four corners (in section) form the points of a rectangle. The potential function ψ is given by an equation of the form

$$x + yi = A(\phi + \psi i) + B \frac{1}{H(\phi + \psi i)}, \quad . \quad . \quad . \quad (4)$$

where $H(u)$ represents the function developed by Jacobi in the *Fundamenta Nova*, p. 172, of the numerator of $\sin am u$. The

* Pogg. *Ann.* vol. lxxxvi.

strips correspond, according to the system there adopted, to the values $\phi = \pm 2K$, where $x = \pm 2AK$ gives half the distance of the strips, while the breadth of the strips depends upon the relation between the constants A and B.

From the form of the equations (2) and (4) we may see that ϕ and ψ can only be expressed as functions of x and y by exceedingly complicated serial development.

XLIV. *On Unitation ; a novel Arithmetical Operation.*

By W. H. WALENN, F.C.S.*

IT has long been known that the remainder to the division of any number by 9 may be determined by dividing the sum of its digits by 9. The following theorem is a generalization and explanation of this property, and establishes a similar operation for all other divisors less than 10.

The theorem is, that if t be the tens' and u the units' digit of a two-figure number, and δ be any integer less than 10, then

$$\overline{10 - \delta t + u}$$

has the same remainder to δ as $10t + u$.

For $\overline{10 - \delta t + u} = 10t + u - \delta t$, and the latter expression is only different from $10t + u$ by an exact number of times δ .

Further, if the digits be s, t, u , the same is true of

$$\overline{10 - \delta^2 s + \overline{10 - \delta t + u}},$$

and so on. For, each time 10 occurs as a factor in any term, it must be treated in the way above indicated, and

$$\overline{10 - \delta^2 s + \overline{10 - \delta t + u}} = \overline{10 - \delta} \overline{10 - \delta s + \overline{10 - \delta t + u}}.$$

These formulæ show the means of determining the remainder to any digit without knowledge of any multiple of that digit, inasmuch as the operation shown by the formula $\overline{10 - \delta t + u}$ only involves multiplication and addition (when $\delta = 9$, addition only); instead of dividing the result of the said operation by δ to obtain the final result, the operation is repeated until only one digit, necessarily one or more units, is obtained. The fitness of regarding this method of obtaining the remainder to δ as an operation totally distinct from that of division, and the fact of single digits only being used, make it convenient to call the said operation "unitation," the remainders being "unitates," and the divisor the "base."

In practice, unitation itself may be simplified by commencing

* Communicated by the Author.

at the left hand and reducing the result of the operation to a single figure as soon as any number higher than 9 arises; for instance, to unitate 34567 to the base 8, or, as it might be expressed, to find $U_8 34567$, the work is $6+4=10$, $4+5=9$, $2+6=8$, 7; $\therefore U_8 34567=7$.

Regarding unitation in connexion with the well-known fact that all direct operations upon the remainders to divisors are respectively analogous to the same operations upon the dividends, also taking into account the periodicity of series of unitates—the verification of direct arithmetical operations and of formulæ involving them (with a certain degree of accuracy) becomes very easy; also the unitate of any unknown number may be calculated from a known number with which it is connected by a known direct operation, or by some inverse operations.

In the following examples, Ux = the unitate of x to the base 9.

Examples:—I. $x=25(2558^2-187^3)=104025$.

$$Ux = U\{7(2^2 - U7^3)\} = U\{7(4-1)\} = U(7 \times 3) = 3.$$

$$\text{II.} \quad x = 537 \cdot 2 \times 23 = 12355 \cdot 6.$$

$$Ux = U(8 \times 5) = 4.$$

$$\log x = 2 \cdot 7301360 + 1 \cdot 3617278 = 4 \cdot 0918638.$$

$$U(\log x) = U(4+8) = 3.$$

$$\text{III.} \quad x = \frac{36326599}{1342} = 27069 + \frac{1}{1342}.$$

$$Ux = 7 = 6 + 1;$$

or, proving by multiplication,

$$x_l = 36326599 = (27069 \times 1342) + 1.$$

$$Ux_l = 7 = (6 \times 1) + 1.$$

$$\text{IV.} \quad x = 58^{247}; \text{ find } Ux, U_8x, \text{ and } U_7x.$$

In this case $Ux^y = U\{Ux^{Uy}\}$, for the series of the powers of $U58$, =4, has 3 (a divisor of 9) as its repeating period; $\therefore Ux = U4^4 = 4$; *i. e.* $\frac{58^{247}}{9}$ gives 4 as a remainder. $U_8x^y = U_8\{Ux^{Uy}\}$ being true for U_858^{247} , $U_8x = U_82^7 = 8$, that is, 58^{247} is divisible by 8 without a remainder. $U_7x = 2^1 = 2$, for the series of the powers of U_758 has 6 for its repeating period.

V. $x = 47^{85}$; find Ux , U_8x , and U_7x . The series Ux_l^n , Ux^{n+1} , Ux_l^{n+2} , &c. repeats after 6 terms; except when $Ux_l = 2$, 5, or 8, the same series repeats after 9 terms. $U47 = 2$, it therefore repeats after 6 terms, and $Ux = 2^1 = 2$, $U_8x = U_87^5 = 7$, $U_7x = 5^1 = 5$.

VI. The series Ux^2 , giving x the successive values 1, 2, 3, 4, 5, 6, 7, 8, 9, is 1, 4, 9, 7, 7, 9, 4, 1, 9. This series enables Tables of squares to be verified within certain limits of correctness.

VII. A similar series for Ux^3 is 1, 8, 9, 1, 8, 9, 1, 8, 9.

VIII. A similar series for Ux^{-1} is (excluding the values of $x=3, 6$, and 9) 1, 5, —, 7, 2, —, 4, 8, —. This series enables unitates to be assigned to such circulating decimals as $\frac{1}{7}$.

The operations U_8x , U_9x , $U_{10}x$, $U_{11}x$, $U_{99}x$, and $U_{999}x$ are the most useful and easily performed.

When any series of numbers, the powers of natural numbers for instance, is to undergo examination by unitation, it is useful to make a rectangular Table with the unitates of the powers in vertical columns; each vertical column belonging to one power—the square for instance. The unitates of the powers of the roots are in horizontal columns, each horizontal column referring to the powers of the same root—2 for instance.

XLV. On Riccati's Equation. By A. CAYLEY, F.R.S.*

THE following is, it appears to me, the proper form in which to present the solution of Riccati's equation.

The equation may be written

$$\frac{dy}{dx} + y^2 = x^{2q-2},$$

which is integrable by algebraic and exponential functions if $(2i+1)q = \pm 1$, i being zero, or a positive integer. To effect the integration, writing $y = \frac{1}{u} \frac{du}{dx}$, we have

$$\frac{d^2u}{dx^2} = x^{2q-2}u.$$

The peculiar advantage of this well-known transformation has not (so far as I am aware) been explicitly stated; it puts in evidence the form under which the sought-for function y contains the constant of integration. In fact if $u=P$, $u=Q$ be two particular solutions of the equation in u , then the general solution is $u=CP+DQ$; and denoting by P' , Q' the derived functions, the value of y is

$$y = \frac{CP' + DQ'}{CP + DQ},$$

* Communicated by the Author.

showing the form under which the constant of integration $C \div D$ is contained in y . To complete the solution, assume

$$u = ze^{\frac{1}{q}x^q};$$

we find

$$\frac{d^2z}{dx^2} + 2x^{q-1} \frac{dz}{dx} + (q-1)x^{q-2}z = 0 :$$

considering first the particular integral of the form

$$z = A + Bx^q + Cx^{2q} + Dx^{3q} + \&c.,$$

we find that the equation will be satisfied if

$$\begin{aligned}(q-1)A + q(q-1)B &= 0, \\ (3q-1)B + 2q(2q-1)C &= 0, \\ (5q-1)C + 3q(3q-1)D &= 0, \\ (7q-1)D + 4q(4q-1)E &= 0, \\ \&c.\end{aligned}$$

If $A=1$, this is

$$\begin{aligned}A &= 1, \\ B &= -\frac{q-1}{q(q-1)}, \\ C &= +\frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}, \\ D &= -\frac{(q-1)(3q-1)(5q-1)}{q(q-1)2q(2q-1)3q(3q-1)} \\ \&c.,\end{aligned}$$

where it is to be noticed that the series may be considered to stop so soon as there is in the numerator a factor $=0$. For instance, if $5q-1=0$, then if the particular integral had been assumed to be $z=A+Bx^q+Cx^{2q}$, the only conditions to be satisfied by the coefficients are the first and second equations giving the foregoing values of A, B, C . It is immaterial that the analytical expressions of F and the subsequent coefficients contain in the denominators the evanescent factor $5q-1$; the coefficients after C do not ever come into consideration.

Thus if $(2i+1)q = +1$, the series terminates, and we have for u the finite particular solution

$$u = P = \left(1 - \frac{q-1}{q(q-1)}x^q + \frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)}x^{2q} - \&c.\right)e^{\frac{1}{q}x^q} :$$

and it is easy to see that we may herein change the sign of x^q , thereby obtaining another finite particular solution,

$$u = Q = \left(1 + \frac{q-1}{q(q-1)} x^q + \frac{(q-1)(3q-1)}{q(q-1)2q(2q-1)} x^{2q} + \&c.\right) e^{-\frac{1}{q} x^q}.$$

Reverting to the equation in z , we have next a particular solution of the form

$$z = Ax + Bx^{q+1} + Cx^{2q+1} + Dx^{3q+1} + \&c.,$$

giving between the coefficients the relation

$$\begin{aligned}(q+1)A + (q+1)q B &= 0, \\ (3q+1)B + (2q+1)2qC &= 0, \\ (5q+1)C + (3q+1)3qD &= 0, \\ (7q+1)D + (4q+1)4qE &= 0, \\ \&c.\end{aligned}$$

If $A=1$, we have

$$\begin{aligned}A &= 1, \\ B &= -\frac{(q+1)}{(q+1)q}, \\ C &= +\frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q}, \\ D &= -\frac{(q+1)(3q+1)(5q+1)}{(q+1)q(2q+1)2q(3q+1)3q}, \\ \&c.,\end{aligned}$$

where, as in the former case, the series is considered to terminate as soon as there is an evanescent factor in the numerator, without any regard to the subsequent coefficients which contain in the denominators the same evanescent factor.

Hence if we have $(2i+1)q = -1$, the series terminates, and we have for u the finite particular solution,

$$u = P = x \left(1 - \frac{q+1}{(q+1)q} x^q + \frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q} x^{2q} - \&c.\right) e^{\frac{1}{q} x^q},$$

from which, changing the sign of x^q , we deduce the other finite particular solution,

$$u = Q = x \left(1 + \frac{q+1}{(q+1)q} x^q + \frac{(q+1)(3q+1)}{(q+1)q(2q+1)2q} x^{2q} + \&c.\right) e^{-\frac{1}{q} x^q}.$$

Hence, in the equation

$$\frac{dy}{dx} + y^2 = x^{2q-2},$$

where $q(2i+1) = \pm 1$, we have (writing $D=1$)

$$y = \frac{CP' + Q'}{CP + Q},$$

where C is the constant of integration, P, Q are finite series as above, and P', Q' are the derived functions of P and Q . Writing successively $i=0, i=1, i=2$, &c., we may tabulate the solutions

$$\frac{dy}{dx} + y^2 = 1, \quad P = e^x, \quad Q = e^{-x},$$

$$\frac{dy}{dx} + y^2 = x^{-4}, \quad P = xe^{-\frac{1}{x}}, \quad Q = xe^{\frac{1}{x}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{4}{3}}, \quad P = (1 - 3x^{\frac{1}{3}})e^{3x^{\frac{1}{3}}}, \quad Q = (1 + 3x^{\frac{1}{3}})e^{-3x^{\frac{1}{3}}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{8}{3}}, \quad P = x(1 + 3x^{-\frac{1}{3}})e^{-3x^{-\frac{1}{3}}}, \quad Q = x(1 - 3x^{-\frac{1}{3}})e^{3x^{-\frac{1}{3}}},$$

$$\frac{dy}{dx} + y^2 = x^{-\frac{8}{5}}. \quad P = \left(1 - 5x^{\frac{1}{5}} + \frac{25}{3}x^{\frac{2}{5}}\right)e^{5x^{\frac{1}{5}}}, \quad Q = \left(1 + 5x^{\frac{1}{5}} + \frac{25}{3}x^{\frac{2}{5}}\right)e^{-5x^{\frac{1}{5}}},$$

&c.

It is hardly necessary to make the final step of calculating P' and Q' and substituting in y ; but, as an example, take the above equation $\frac{dy}{dx} + y^2 = x^{-\frac{4}{3}}$: we have

$$y = \frac{-3x^{-\frac{1}{3}}(Ce^{3x^{\frac{1}{3}}} + e^{-3x^{\frac{1}{3}}})}{C(1 - 3x^{\frac{1}{3}})e^{3x^{\frac{1}{3}}} + (1 + 3x^{\frac{1}{3}})e^{-3x^{\frac{1}{3}}}},$$

which is readily identified with the solution, p. 98 of Boole's 'Differential Equations' (Cambridge, 1859).

Cambridge, September 29, 1868.

XLVI. *On the New Electromotive Force in the Voltaic Arc.*

By E. EDLUND*.

WHEN a luminous arc is formed between two charcoal points by means of a powerful voltaic battery, and the circuit is broken in any place on one side of the arc so that the current ceases, and if the circuit be completed after a brief interval, the luminous arc is not extinguished, though the current was interrupted for a short time. The time during which the current may be thus interrupted without extinguishing the arc depends upon the intensity of the current. With a strong current the time may be longer; with a weak one the time must be reduced if the arc is to continue. If the same experiment be made with a luminous arc between silver instead of carbon poles, the light is extinguished at the moment of interruption, and does not spontaneously reappear when the circuit has again been restored. This deportment of the light between carbon poles doubtless indicates that the conduction in the arc lasts a short time after the cessation of the current, by which it is possible that the current, when the break on the side of the luminous arc has been restored, can reproduce the arc. If the conduction had been quite destroyed during the short break, the incipient current would not be able to traverse the path between the carbon poles and renew the light. But the conduction in the luminous arc is principally produced by detached particles which are carried by the current from one pole to the other. It must hence be assumed that in the first moment after the break in the conduction the particles of carbon are being continually detached and carried from one pole to the other.

In a previous investigation I have proved that the mechanical work which the current consumes in order to disintegrate the poles gives rise to an electromotive force which sends a current in the opposite direction to the principal current †. If, now, this disintegration lasts for a short time after the cessation of the principal current, it may be possible to separate the current resulting from this electromotive force from the principal one, and measure it free from all action of the latter. With this design I have made the following experiments in common with Dr. Lenström, of Helsingfors.

2. To break the circuit on the side of the arc, a commutator

* Translated from a separate copy communicated by the Author, having been read before the Royal Academy of Sciences at Stockholm, January 1868.

† Phil. Mag. S. 4. vol. xxxv. p. 103.

current. When in reversing the lever the fork-points n and n' are raised, the surface of the mercury rises about the points, owing to adhesion; and therefore the contact between them lasts longer than if the surface of the mercury remained unchanged. If the current is passing, a formation of sparks ensues as soon as contact between the mercury and the fork-points ceases; and as long as the formation of sparks lasts, the current continues. This retardation in the cessation of the current, due to the formation of sparks, increases with the intensity of the current. The reversal of the lever was effected by means of a falling weight, in order that it might always take place with the same speed. The time which elapsed from the moment in which the forked points left the mercury in the cups n and n' to the moment in which the points at the other end of the lever touched the mercury in the cups l and l' , was calculated to amount to $\frac{1}{80}$ of a second. Yet, owing to the formation of sparks and the adhesion of the mercury, the time that elapsed between the cessation of the current and junction between l and l' was much shorter. That in this rapid reversal the junction between n and n' was not simultaneous with that between l and l' , was proved in the following manner. The pole-points at b were pushed together, the conducting-wire p taken away, and in its place a German-silver wire of considerable resistance was interposed between n' and l' . This resistance, however, was so arranged as to produce a smaller diminution in the intensity of the current than the luminous arc when this existed and n' was connected with l' by p . Thereupon the magnetometer-wire $g g$ was detached from q and joined with n' . If, now, the closing had been simultaneous on reversing the lever, part of the principal current must have gone through the magnetometer and have produced a deflection; in the opposite case the needle must have remained at rest. In this experiment the intensity of the current was greater than in the following, and accordingly the formation of sparks more intense. Yet notwithstanding this, there was no simultaneous closing. In order to diminish in any possible way the formation of sparks, the mercury in the cups n and n' was covered with a thin layer of oil. That there was no simultaneous closing follows also from another experiment, which will be mentioned afterwards.

3. We pass now to the actual observations, which, however, are not adduced in the order in which they were made. As the resistance in the luminous arc is great and increases with its length (other things remaining the same), the deflection must have been smaller in proportion as the length of the arc increased. Hence it was important to have the same length of arc in the various experiments. For this purpose one carbon point was moveable, and might be pushed backwards and forwards by means of a

micrometer-screw. Notwithstanding this, the length of the arc was not the same in all the experiments, in consequence of which the deflections varied not inconsiderably. It was necessary, therefore, to make a greater number of experiments and take the mean. To give an idea of the magnitude of the variations, with the mean numbers adduced below are given both the probable errors and the number of observations.

Experiment 1. The battery consisted of fifty Bunsen's elements with carbon and nitric acid. The trough described in my former investigations was placed in the circuit, and, as before, was partially filled with solution of sulphate of copper. The distance between the copper disks was 5 inches. By this resistance the intensity of the current was materially diminished. The luminous arc was formed between carbon points. When the magnetometer-wires were connected in the above manner with the path of the circuit, on reversing the lever there was always a deflection in the galvanometer, which indicated a current between the points in the opposite direction to the principal current. As the mean of ten observations, this deflection amounted to 18·7 divisions, with a probable error of $\pm 1\cdot24$.

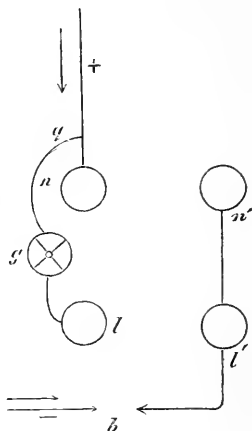
When the carbon points were replaced by similar ones of silver and the same experiment repeated, the magnetic needle remained completely at rest. When, therefore, the luminous arc was formed between silver poles, there was no subsequent action after the arc was extinguished. This deportment proves that the cause of the current which produces the deflection is to be sought in the luminous arc itself, because the deflection depends upon the quality of the pole-points. This proves at the same time that there was no simultaneous closing between n, n' and l, l' .

The mean deflection, = 18·7, adduced above gives of itself no idea of the magnitude of the electromotive force by which the current is produced. The deflection depends not merely on the delicacy of the instrument and the resistance between the poles, but also on the time during which the current acts, and on the electromotive force. Without information on this point, it cannot be determined beforehand whether the electromotive force is most nearly equal, say, to that of 0·1 of an element, or of from ten to fifteen such elements. In order to determine the force in question the following method was pursued.

Experiment 2. The connexion between the principal current, the magnetometer, and the commutator was altered, as represented in fig. 2. The mercury-cup n was joined with the wire from the positive pole of the battery, and the magnetometer-wire $g g$ connected with the same wire. The conducting-wire from the negative pole of the battery was directly connected with one carbon point. When n and n' were connected by the

fork-shaped lever, the current formed a luminous arc at b ; when, on the contrary, the lever was reversed by the falling weight, contact between n and n' ceased, the luminous arc was extinguished, and the current passed through the galvanometer so long as the conduction lasted through the extinguishing luminous arc. The deflection obtained was produced by the electromotive force of the battery, diminished by the electromotive force in the extinguishing luminous arc. The battery consisted of the same fifty elements as in experiment 1, and with the same resistance in the solution of sulphate of copper. When this resistance was not introduced, the current was so strong that the luminous arc, as mentioned before, was not extinguished when the lever was reversed.

Fig. 2.



As the mean of ten observations, a deflection of 27.9 divisions was obtained, with a probable error of ± 1.43 . With a luminous arc between silver points no deflection was obtained, the needle was quite stationary. Hence between the silver poles there was no conduction when the luminous arc was extinguished.

From the deflections obtained (18.7 and 27.9) the electromotive force in the disappearing arc cannot be directly calculated, because the resistance is unequal in the two cases. In the first case it consists of that in the arc, together with that of the magnetometer. In the latter case there is in addition the resistance of the battery and the solution of sulphate of copper. Calling the first m and the latter m' , the electromotive force of the battery E , and the electromotive force in the extinguishing luminous arc x , we have

$$\frac{E-x}{m} : \frac{x}{m'} = 27.9 : 18.7.$$

In the following experiments a determination is obtained of the ratio between m' and m , by the aid of which the calculation can be executed.

Experiment 3. The battery consisted of twenty-six elements, and the sulphate solution was not interposed in the circuit. When the observations were made as in experiment 1, a deflection of 40 divisions was obtained as the mean of fifteen observations, with a probable error of ± 3.5 . When afterwards a German-silver wire 18.8 feet long was introduced, a deflection

of 34 divisions was obtained as the mean of fifteen observations, with a probable error of ± 3.7 . From this the resistance in the disappearing luminous arc may be calculated. If the resistance in the German-silver wire is ρ , and that in the luminous arc and the magnetometer together is called B, we have

$$B : B + \rho = 34 : 40,$$

from which $B = 5.67\rho$.

Experiment 4. With thirty elements, but without introducing the sulphate of copper into the circuit, fifteen observations were made as in experiment 2. The battery was increased from twenty-six to thirty elements, because the luminous arc was so feeble with twenty-six elements that it was often spontaneously extinguished, in consequence of which the experiments had to be frequently repeated. The mean of these fifteen observations was 64 divisions, with a probable error of ± 6.5 . The resistance in the battery was found on measurement to amount to 1.7ρ . The resistance in the luminous arc with the use of twenty-six elements was found to be 5.67ρ ; when thirty elements are used the resistance becomes somewhat smaller, for it diminishes as the intensity increases. The resistance in the entire circuit was somewhat smaller than $1.7\rho + 5.67\rho = 7.37\rho$. If the resistance had been equal to that in experiment 3, the deflection of the magnetometer would at most have been

$$\frac{64 \times 7.37}{5.67} = 83.2 \text{ divisions.}$$

Hence we have

$$83.2 : 40 = E - x : x,$$

from which $x = 9.7$ elements.

We see from this that the electromotive force in the extinguishing luminous arc between carbon points cannot be less than that of 9.7 Bunsen's elements.

The data furnished by this and by previous experiments may be used to calculate the electromotive force of the luminous arc from experiments 1 and 2.

If the resistance in 30 elements is 1.7ρ ,

It may be assumed that in 50 elements it is $\frac{50}{30} 1.7\rho$, or 2.83ρ

If the resistance in one inch of the solution of sulphate of copper be called r , a special investigation showed that $\rho = 2.06r$;

Hence in five inches of the solution of sulphate of } 2.43ρ
copper the resistance was }

Hence the total resistance in the battery and in the rheostat together is 5.26ρ . Assuming that the resistance in the luminous arc and in the magnetometer, as in experiment 3, amounts

to 5.67ρ , the entire resistance in the battery, in the rheostat, in the arc, and in the magnetometer together amounts to 10.93ρ .

If the resistance in experiment 2, as in experiment 1, had been equal to 5.67ρ , the deflection of the magnetic needle must have been

$$\frac{10.93}{5.97} \times 27.9 = 53.8 \text{ divisions.}$$

Hence we have

$$53.8 : 18.7 = 50 - x : x,$$

from which $x = 12.9$ elements.

Experiment 5. The battery consisted of fifty elements; and five inches of the solution of sulphate of copper were introduced into the circuit. The resistance of the battery was found to be 3.97ρ , which, together with the resistance in the circuit, makes 6.4ρ . When the galvanometer circuit surrounded the luminous arc, the deflection on reversing the lever was 11.7 divisions as the mean of ten observations, with a probable error of ± 0.85 . When thereupon a resistance $= 12.9\rho$ was introduced into the circuit, the deflection was equal to 6.5 divisions as the mean of ten observations, with a probable error of ± 0.37 . From this it may be calculated that the resistance in the luminous arc and the magnetometer together was equal to 16.1ρ . The great increase in the resistance of the luminous arc was probably due to a smaller intensity of the current.

Experiment 6. The battery and the interposed resistance were the same as in experiment 5. The magnetometer-wire was connected with the commutator and the principal circuit as represented in fig. 2. When the lever was reversed, a deflection was always obtained amounting to 19.2 divisions as the mean of fifteen observations, with a probable error of ± 1.35 . The resistance in this case was $= 22.5\rho$ ($= 6.4\rho + 16.1\rho$). If the resistance had been equal to 16.1ρ , as in experiment 5, the deflection would have been $= 26.83$ divisions. We have thus

$$26.83 : 11.7 = 50 - x : x,$$

from which $x = 15.2$ elements.

Hence the mean of the three determinations adduced of the electromotive force of the vanishing luminous arc amounts to 12.6 elements. The probable errors adduced above show that this determination can lay claim to no great accuracy. It is possible that the true value may be a few units above or below the one adduced; but this is immaterial for our immediate object. The experiments adduced prove that, in the voltaic arc between carbon points, during the first moment after extinction there is an electromotive force comparable in magnitude to that of from ten to

fifteen Bunsen's elements. It is moreover to be remarked that the numbers obtained are the means of the electromotive force. This is doubtless strongest during the first moment after the extinction of the luminous arc, and then diminishes in intensity till it quite ceases. If the lever could be more quickly reversed than has been heretofore possible, and thereby let the closing of the magnetometer circuit follow more rapidly the opening of the principal circuit and the extinction of the luminous arc, a higher value would be obtained for the electromotive force of the luminous arc.

4. The source of electricity in the disappearing luminous arc cannot be of the nature of an electromotive force due to contact. There are indeed two contacts between carbon and gas; but these act in opposite directions and neutralize each other. It is possible that when the experiments are made in air, the moisture present is decomposed and produces a polarization of the carbon points; but even if this were the case, it would only explain an insignificant part of the electromotive force found. Just as little can the force be an induction-phenomenon; for the conditions for the production of such a one do not exist in the luminous arc. It also seems to have nothing in common with the electromotive force discovered by Quincke to accompany the passage of liquids through porous diaphragms. The only supposition which might possibly be made is that the force is of thermoelectric origin. The positive pole is considerably more heated than the negative; and we have thus in the luminous arc two contacts between carbon and gas, of which one has a considerably higher temperature than the other. But it appears improbable that this thermoelectric force could have obtained so considerable a value as that we have observed*. In order to ascertain with certainty the real state of the case, the following experiments were made.

If the force in question is of thermoelectric origin, and caused by the circumstance that the positive pole has a higher temperature than the negative one, the current observed must disappear when the temperature of the negative pole is raised to the same

* Mr. Wilde, in *Pogg. Ann.* vol. cxi., has communicated a paper in which it is demonstrated that a delicate magnetometer gives a considerable deflection if it is connected with a battery of twenty Bunsen's elements as in fig. 1. Mr. Wilde does not mention any precautions which he used to convince himself that in his experiments there was no simultaneous closing on opening the principal current and introducing the magnetometer circuit. If there is a simultaneous closing, deflections of the galvanometer are always obtained; but these are then caused by the principal current itself. Mr. Wilde has made no experiments with the view of measuring the strength of the current or the intensity of its electromotive force. Hence Mr. Wilde did not hesitate to assume that the deflections obtained were caused by a thermoelectric current which resulted from the contact of the pole points (heated to unequal temperatures) with the interjacent gas.

height as that of the positive. If, also, the temperature of the negative pole be only so much raised that the difference between the poles is decreased, the thermoelectric current must be diminished; or it might even be possible that it would sink to zero, and then acquire an opposite direction, before the temperature of the negative pole attained that of the positive. If the statement that the current decreases when the temperature of the negative pole is increased is perfectly correct under all circumstances, the arc must of course retain its conductivity unchanged. Yet this depends principally, if not exclusively, on the quantity and condition of the transported particles, and not on the temperature of the gas. On the contrary, it is by no means certain *à priori* that the current will increase if the temperature of the positive pole be raised, and thereby the difference between the temperature of the poles become greater, because it is possible that the greatest electromotive force corresponded to a certain temperature of the positive pole, and that the force would become less at a higher temperature. It is also important to be able to show that increase in temperature in the negative pole produces no diminution in the intensity of the current.

To effect this, a Bunsen's burner provided with a bellows was used. When the flame was directed against carbon of the same quality as that used in the experiments, the carbon began to ignite, and a thin wire coiled round it melted. The temperature, at any rate on the side of the carbon turned towards the flame, was 1000° or more. Upon this the following experiments were made, partly without heating by the burner, partly with the application of heat to the positive or negative pole as near as possible to the arc.

Experiment 7. The battery consisted of fifty elements; but the current, probably owing to imperfect conduction, was not particularly strong. The magnetometer was connected with the principal circuit as shown in fig. 1; that is, the electromotive force in the luminous arc caused the deflection.

The mean of ten observations, when there was no heating, and when the negative pole was heated to strong ignition, gave:—

Without heating	30.8 divisions.
The probable error	± 2.5 .
Heating the negative pole . .	40.8 divisions.
The probable error	± 3.1 .

On heating the negative pole, therefore, and diminishing thus the difference in temperature between the poles, the current was stronger—quite opposite to what should have been the case if the current had been of thermoelectric origin. In the subsequent

experiments the positive and negative poles were alternately heated.

Experiment 8. With a somewhat stronger current than in the previous experiments, there was obtained as the mean of ten observations, on

Heating the positive pole	40 divisions.
Probable error	± 3.1 .
Heating the negative pole	54 divisions.
Probable error	± 3.8 .

The current was also greater in this case when the difference in temperature was diminished.

Experiment 9. The battery consisted of thirty elements without any introduced external resistance. There was obtained as the mean of fifteen observations,

Without heating	46 divisions.
Probable error	± 2.8
Heating the positive pole	36.5 divisions.
Probable error	± 3.1 .
Heating the negative pole	45.3 divisions.
Probable error	± 2.2 .

Experiment 10. The battery consisted of fifty elements, with no other resistance than that afforded by the thick conducting-wires. The current was strong and the deflections great; but the difficulty of obtaining constant deflections increased in the same proportion. There was obtained as the mean of fifteen observations,

Heating the positive pole	73 divisions.
Probable error	± 7.7 .
Heating the negative pole	73 divisions.
Probable error	± 7.1

Although, as the probable errors adduced indicate, the sources of error are great, the preceding experiments justify the conclusion that the current in question does not decrease when the difference in temperature between the poles is less, owing to the heating of the negative pole, which it ought to do if the current were of thermoelectric origin. To this may be added that this diminution of the difference of temperature is very considerable, although it certainly does not entirely disappear.

When the positive pole is heated, the deflection seems to be somewhat less—though this cannot be considered to be made

out, for the experiments which speak in its favour are not more numerous or more accurate.

Moreover the result which these heatings of the poles give is nothing more than a confirmation of the conclusions which I have drawn from my earlier investigation on the voltaic arc. No one will doubt that the electromotive force which, from the experiments adduced, is met with in the just-extinguished arc is the same as that which, from my former investigations, occurs in the luminous arc during its existence. But it has been shown* in regard to this force, that it is independent of the intensity of the current. It is only when the intensity approaches the minimum with which a luminous arc can be produced, that a decrease in the magnitude of the force begins to be seen†. But the temperature of the pole-points is dependent on the intensity of the current, and the same is also obviously the case with their difference in temperature. The force found is thus independent of the heating of the poles, and therefore cannot be of thermoelectric origin.

XLVII. *On Geological Time, and the probable Date of the Glacial and the Upper Miocene Period.* By JAMES CROLL, of the Geological Survey of Scotland.

[Continued from p. 154.]

AS has already been remarked, if the glacial epoch resulted from a high state of excentricity there must, according to the foregoing Tables, have been at least five ice-periods during the last three millions of years, viz. the glacial epoch, two as severe, and two much less severe. The question which now remains to be discussed is whether this conclusion is consistent with the facts of geology or not. Can it be shown from geological evidence that there have not within the past three millions of years been two ice-periods as severe as the glacial period? In other words, do the testimony of geology and the testimony of physics agree in regard to this point?

From what has already been shown respecting the limit to geological time, I presume we may reasonably conclude that three millions of years may probably embrace the greater portion of the Tertiary period. Now, as has been stated, we have good evidence of at least three ice-periods since the beginning of the Tertiary period—one about the middle of the Eocene period, another during the Upper Miocene period, and the third and last the well-known glacial epoch.

* Phil. Mag. S. 4. vol. xxxv. p. 103.

† *Öfversigt af K. Vet. Akad. Förhandl.* December 1867.

Let it be observed that it is not essential to the object in view to be able to show from the facts of geology that former cold periods, such as those of the Eocene and the Miocene, were as severe as that of the glacial epoch. All that is required is simply to show that, for anything that geology can prove to the contrary, those cold periods *may* have been as severe as the glacial epoch. What I wish to show is, that although the conclusions derived from astronomical and physical considerations regarding the severity of former ice-periods may not at present be fully borne out by geological science, the imperfection of geological records upon this point is such that the absence of direct geological evidence cannot reasonably be regarded as sufficient proof that the physical conclusions arrived at are improbable. But this is not all; I shall endeavour to show that the records of geology in regard to former glacial epochs are not only imperfect, but that this imperfection *follows as a necessary consequence from the principles of geology itself*—that there is not simply a want of records, but a *reason* in the very nature of geological evidence why there is such a want.

Geological evidence in reference to past glacial epochs is evidently much more imperfect than many suppose.

It is on a land-surface that the principal traces of the action of ice during a glacial epoch are left; for it is there that the stones are chiefly striated, the rocks ground down, and the boulder-clay formed. But where are all our ancient land-surfaces? They are not to be found. The total thickness of the stratified rocks of Great Britain, according to Professor Ramsay, is nearly fourteen miles. But from the top to the bottom of this enormous deposit there is hardly a single land-surface to be found. True, there are patches of old land-surfaces of a local character, such, for example, as the dirt-beds of Portland; but, with the exception of the coal-beds, every general formation from top to bottom was formed under water, and none but the under-clays *ever existed as a land-surface*. And it is here in such a formation that the geologist has to collect all his information regarding the existence of former glacial epochs. The entire stratified rocks of the globe, with the exception of the coal-beds and under-clays (places where no one would expect to find traces of ice-action), consist almost entirely of a *series of old sea-bottoms*, with here and there an occasional freshwater deposit. Bearing this in mind, what is the sort of evidence which we can now hope to find in these old sea-bottoms of the existence of former ice-periods?

Every one, of course, who has ever reflected on the matter admits that the stratified rocks are not old land-surfaces, but a series of old sea-bottoms formed out of the destruction of old

land-surfaces. And it is true that all land-surfaces once existed as sea-bottoms; but the stratified rocks consist of a series of old sea-bottoms which never were land-surfaces. Many of them no doubt have been repeatedly above the sea-level, and may once have possessed land-surfaces; but these, with the exception of the under-clays of the coal-measures, the dirt-beds of Portland, and one or two more patches, have all been annihilated. The important bearing that this consideration has on the nature of the evidence which we can now expect to find of the existence of former glacial epochs has certainly been very much overlooked.

When we examine the matter fully, we find that the *transformation of a land-surface into a sea-bottom* will probably completely obliterate every trace of glaciation that may have existed on that land-surface. For example, we cannot expect to find the polished and striated stones belonging to a former land-glaciation; for stones are not carried down by our rivers and deposited in the sea. They are first disintegrated by subaërial agency into sand or clay, as the case may be, and then carried down by our rivers and deposited as such on the sea-bottom. But supposing striated stones were carried down by our rivers out of the boulder-clay, they could not retain their ice-markings, for they would soon become waterworn in their passage seawards.

Neither can we expect to find boulder-clay in the stratified rocks, for boulder-clay is not carried down as such and deposited in the sea. The boulder-clay is washed off the land and carried down as soft mud, clay, sand, and gravel. Patches of boulder-clay may have been now and again forced into the sea by means of the ice and become covered up; but such cases are wholly exceptional, and the absence of examples of this sort in any formation cannot fairly be adduced as a proof that that formation does not belong to a glacial period.

The only evidence which we can now reasonably expect to find in the stratified rocks of the existence of land-ice of former epochs, is the presence of erratic blocks which may have been transported by icebergs and dropped into the sea. But unless the glaciers of that epoch reached the sea or the sea was frozen, we could not possibly have even this evidence. Traces in the stratified rocks of the effects of land-ice of former epochs must, from the very nature of things, be rare indeed. The only sort of evidence which, as a general rule, we may always expect to find is the presence of large blocks of older rocks found imbedded in strata which, we know from their constitution, must have been formed in still water. But this is quite enough; for it proves the existence of ice at the time that the deposit containing the blocks was being formed as conclusively as though we saw the ice floating with the blocks

upon it. This sort of evidence, when found in low latitudes, ought to be received as conclusive of the existence of former glacial epochs, and, no doubt, would have been so received had it not been for the idea that, if these blocks had been transported by ice, there ought in addition to have been found striated stones, boulder-clay, and other indications of the agency of land-ice.

The reason why we now have, comparatively speaking, so little direct evidence of the existence of former glacial periods will be more forcibly impressed upon the mind, if we reflect on how difficult it would be in a million or so of years hence to find any trace of what we now call the glacial epoch. The striated stones would by that time be all, or nearly all disintegrated, and the till washed away and deposited in the bottom of the sea as stratified sands and clays. And when these became consolidated into rock and were raised into dry land, the only evidence that we should probably then have that there ever had been a glacial epoch would be the presence of large blocks of the older rocks, which would be found imbedded in the upraised formation. We could only infer that there had been ice at work from the fact that by no other known agency could we conceive such blocks to have been transported and dropped in a still sea.

Probably few geologists believe that during the Middle Eocene and the Upper Miocene periods our country passed through a condition of glaciation as severe as it has done during the Post-pliocene period; yet when we examine the subject carefully, we find that there is actually no just ground to conclude that it has not. For in all probability, in the strata formed out of the destruction of the now existing land-surfaces, evidence of ice-action will be as scarce as in the Eocene or Miocene strata.

If the stratified rocks forming the earth's crust consisted of a series of old land-surfaces instead (as they actually do) of a series of old sea-bottoms, then probably dozens of glacial periods might be detected.

Nearly all the evidence which we have regarding the glacial epoch has been derived from what we find on the now existing land-surfaces of the globe. But probably not a trace of this will be found in the stratified beds of future ages, formed out of the destruction of the present land-surfaces. Even the very arctic shell-beds themselves, which have afforded to the geologist such clear proofs of a frozen sea during the glacial epoch, will not be found in those stratified rocks; for they must suffer destruction along with everything else which now exists above the sea-level. There is probably not a single relic of the glacial epoch which has ever been seen by the eye of man that will be found in the stratified rocks of future ages. Nothing but what

lies buried in the deep recesses of the ocean will escape complete disintegration and appear imbedded in those formations. It is only those objects which lie in our existing sea-bottoms that will remain as monuments of the glacial epoch of the Posttertiary period. And, besides, it will only be those portions of the sea-bottoms that may happen to be upraised into dry land that will be available to the geologist of future ages. The point to be determined now is this:—*Is it probable that the geologist of the future will find in the rocks formed out of the now existing sea-bottoms more evidence of a glacial epoch during Posttertiary times than we now do of one during the Miocene, the Eocene, or the Permian period?* Unless this can be proved to be the case, we have no ground whatever to conclude that the cold periods of the Miocene, Eocene, and Permian periods were not as severe as that of the glacial epoch. This is evident; for the only relics which now remain of the glacial epochs of those periods are simply what happened to be protected in the then existing sea-bottoms. Every vestige that lay on the land would in all probability be destroyed by subaërial agency and carried into the sea in a sedimentary form. But before we can determine whether or not there is more evidence of the glacial epoch in our now existing sea-bottoms than there is of former glacial epochs in the stratified rocks (which are in reality the sea-bottoms belonging to ancient epochs), we must first ascertain what is the nature of those marks of glaciation which are to be found in a sea-bottom.

We know that the rocky face of the country was ground down and striated during the glacial epoch; and this is now generally believed to have been done by land-ice. But we have no direct evidence that the floor of the ocean, beyond where it may have been covered with land-ice, was striated. Beyond the limits of the land-ice it could be striated only by means of icebergs. But do icebergs striate the rocky bed of the ocean? Are they adapted for such work? It seems to be almost universally assumed that they are. But I have been totally unable to find any rational grounds for such a belief. Clean ice can have but little or no erosive power, and never could scratch a rock. To do this it must have grinding materials in the form of sand, mud, or stones. But the bottoms of icebergs are devoid of all such materials. Icebergs carry the grinding materials on their backs, not on their bottoms. No doubt, when the iceberg is launched into the deep, great masses of sand, mud, and stones will be adhering to its bottom. But no sooner is the berg immersed, than a melting process commences at its sides and lower surface in contact with the water; and the consequence is, the materials adhering to the lower surface soon drop off and sink to the bottom of the sea. The iceberg, divested of these materials, can now do very

little harm to the rocky sea-bottom over which it floats. It is true that an iceberg moving with a velocity of a few miles an hour, if it came in contact with the sea-bottom, would, by the mere force of concussion, tear up loose and disjointed rocks, and hurl some of the loose materials to a distance; but it would do but little in the way of grinding down the rock against which it struck. But even supposing the bottom of the iceberg were properly shoed with the necessary grinding materials, still it would be but a very inefficient grinding agent; for a *floating* iceberg would not be in contact with the sea-bottom. And if it were in contact with the sea-bottom, it would soon become stranded and, of course, motionless, and under such conditions could produce no effect.

It is perfectly true that although the bottom of the berg may be devoid of grinding materials, yet these may be found lying on the surface of the subuarine rock over which the ice moves. But it must be borne in mind that the same current which will move the icebergs over the surface of the rock will move the sand, mud, and other materials over it also; so that the markings effected by the ice will in all probability be erased by the current. In the deep recesses of the ocean the water has been found to have but little or no motion. But icebergs always follow the path of currents; and it is very evident that at the comparatively small depth of a thousand feet or so reached by icebergs the motion of the water will be considerable; and the continual shifting of the small particles of the mud and sand will in all probability efface the markings which may be made now and again by a passing berg.

Much has been said regarding the superiority of icebergs as grinding and striating agents in consequence of the great velocity of their motion in comparison with that of land-ice. But it must be remembered that it is while the iceberg is floating, and before it touches the rock, that it possesses high velocity. When the iceberg runs aground, its motion is suddenly arrested or greatly reduced. But if the iceberg advancing upon a sloping sea-bottom is raised up so as to exert great pressure, it will on this account be the more suddenly arrested, the motion will be slow, and the distance passed over short, before the berg becomes stranded. If it exerts but little pressure on the sea-bottom, it may retain a considerable amount of motion and advance to a considerable distance before it is brought to a stand; but, exerting little pressure, it can perform but little work. Land-ice moves slowly, but then it exerts enormous pressure. A glacier 1000 feet in thickness has a pressure on its rocky bed equal to about 25 tons on the square foot; but an iceberg a mile in thickness, forced up on a sloping sea-bottom to an elevation of

20 feet (and this is perhaps more than any ocean-current could effect), would only exert a pressure of about half a ton on the square foot, or about $\frac{1}{50}$ part the pressure of the glacier 1000 feet in thickness. A great deal has been said about the erosive and crushing-power of icebergs of enormous thickness, as if their thickness gave them any additional pressure. An iceberg 100 feet in thickness will exert just as much pressure as one a mile in thickness. The pressure of an iceberg is not like that of a glacier, in proportion to its thickness, but to the height to which it is raised out of the water. An iceberg 100 feet in thickness raised 10 feet will exert exactly the same pressure as one a mile in thickness raised to an equal height.

To be an efficient grinding agent, steadiness of motion, as well as pressure, is essential. A rolling or rocking motion is ill-adapted for grinding down and striating a rock. A steady rubbing motion under pressure is the thing required. But an iceberg is not only deficient in pressure, but also deficient in steadiness of motion. When an iceberg moving with considerable velocity comes on an elevated portion of the sea-bottom, it does not move steadily onwards over the rock, unless the pressure of the berg on the rock be trifling. The resistance being entirely at the bottom of the iceberg, its momentum, combined with the pressure of the current, applied wholly above the point of resistance tends to make the berg bend forward, and in some cases upset (when it is of a cubical form). The momentum of the moving berg, instead of being applied in forcing it over the rock against which it comes in contact, is probably all consumed in work against gravitation in raising the berg upon its front edge. After the momentum is consumed, unless the berg be completely upset, it will fall back under the force of gravitation to its original position. But the momentum which it acquires from gravitation in falling backwards carries it beyond its position of repose in an opposite direction. It will thus continue to rock backwards and forwards until the friction of the water brings it to rest. The momentum of the berg, instead of being applied to the work of grinding and striating the sea-bottom, will chiefly be consumed in heat in the agitation of the water. But if the berg does advance, it will do so with a rocking unsteady motion, which, as Mr. Couthouy* and Professor Dana† observe, will tend rather to obliterate striations than produce them.

A floating berg moves with great steadiness; but a berg that has run aground cannot advance with a steady motion. If the rock over which the berg moves offers little resistance, it may

* Report on Icebergs, read before the Association of American Geologists. Silliman's Journal, vol. xliii. p. 163 (1842).

† Manual of Geology, p. 677.

do so ; but in such a case the berg could produce but little effect on the rock.

Dr. Sutherland, who has had good opportunities to witness the effects of icebergs, makes some most judicious remarks on the subject. "It will be well," he says, "to bear in mind that when an iceberg *touches the ground, if that ground be hard and resisting, it must come to a stand*, and, the propelling power continuing, a slight leaning over in the water, or yielding motion of the whole mass, may compensate readily for being so suddenly arrested. If, however, the ground be soft, so as not to arrest the motion of the iceberg at once, a moraine will be the result ; but the moraine thus raised will tend to bring it to a stand"*.

There is another cause referred to by Professor Dana, which, to a great extent, must prevent the iceberg from having an opportunity of striating the sea-bottom, even though it were otherwise well adapted for so doing. It is this : the bed of the ocean in the track of icebergs must be pretty much covered with stones and rubbish dropped from the melting bergs. And this mass of rubbish will tend to protect the rock†.

If icebergs cannot be shown *à priori*, from mechanical considerations, to be well adapted for striating the sea-bottom, one would naturally expect, from the confident way in which it is asserted that they are so adapted, that the fact has been at least established by actual observation. But, strange as it may appear, we seem to have little or no proof that icebergs actually striate the bed of the ocean. This can be proved from the direct testimony of the advocates of the iceberg theory themselves.

We shall take the testimony of Mr. Campbell, the author of two well-known works in defence of the iceberg theory, viz. 'Frost and Fire' and 'A Short American Tramp.' Mr. Campbell went in the fall of the year 1864 to the coast of Labrador, the Straits of Belle Isle, and the Gulf of St. Lawrence, for the express purpose of witnessing the effects of icebergs, and testing the theory which he had formed, that the ice-markings of the glacial epoch were caused by floating ice and not by land-ice, as is now generally believed.

The following is the result of his observations on the coast of Labrador.

Hanly Harbour, Strait of Belle Isle :—"The water is 37° F. in July. . . . As fast as one island of ice grounds and bursts, another takes its place ; and in winter the whole strait is blocked up by a mass which swings bodily up and down, grating along the bottom at all depths. . . . Examined the beaches and rocks at the water-line, especially in sounds. Found the rocks ground

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Dana's 'Manual of Geology,' p. 677.

smooth, *but not striated*, in the sounds.”—Short American Tramp, pp. 68, 107.

Cape Charles and Battle Harbour:—“But though these harbours are all frozen every winter, the *rocks at the water-line are not striated*” (p. 68).

At St. Francis Harbour:—“The water-line is much rubbed, smooth, *but not striated*” (p. 72).

Cape Bluff:—“Watched the rocks with a telescope, and *failed to make out striæ anywhere*; but the water-line is everywhere rubbed smooth” (p. 75).

Seal Islands:—“*No striæ are to be seen at the land-wash in these sounds or on open sea-coasts near the present water-line*” (p. 76).

He only mentions having here found striations in the three following places along the entire coast of Labrador visited by him; and in regard to two of these, it seems very doubtful that the markings were made by modern icebergs.

Murray’s Harbour:—“This harbour was blocked up with ice on the 20th of July. The water-line is rubbed, and in *some places striated*” (p. 69).

Pack Island:—“The water-line in a narrow sound was polished and striated in the direction of the sound, about N.N.W. This seems to be fresh work done by heavy ice drifting from Sandwich Bay; *but, on the other hand, stages with their legs in the sea, and resting on these very rocks, are not swept away by the ice*” (p. 96). If these markings were modern, why did not the “heavy ice” remove the small fir poles supporting the fishing-stages?

Red Bay:—“Landed half-dressed, and found some *striæ* perfectly fresh at the water-level, but weathered out a short distance inland” (p. 107). The striations “inland” could not have been made by modern icebergs; and it does not follow that because the markings at the water-level were not weathered they were produced by modern ice.

These are the evidences which he found that icebergs striate rocks, on a coast of which he says that, during the year he visited it, “the winter-drift was one vast solid raft of flocs and bergs more than 150 miles wide and perhaps 3000 feet thick at spots, driven by a whole current bodily over one definite course, year after year, since this land was found” (p. 85).

But Mr. Campbell himself freely admits that the floating ice which comes aground along the shores does not produce *striæ*. “It is sufficiently evident,” he says, “*that glacial striæ are not produced by thin bay-ice*” (p. 76). And in ‘Frost and Fire,’ vol. ii. p. 237, he states that, “from a careful examination of the water-line at many spots, it appears that bay-ice grinds rocks, *but does not produce striation*.”

"It is impossible," he continues, "to get at rocks over which heavy icebergs now move; but a mass 150 miles wide, perhaps 3000 feet thick in some parts, and moving at the rate of a mile an hour or more, *appears to be an engine amply sufficient* to account for striæ on rising rocks." And in 'American Tramp,' p. 76, he says, "*striæ must be made* in deep water by the large masses which seem to pursue the even tenor of their way in the steady current which flows down the coast."

Mr. Campbell, from a careful examination of the sea-bottom along the coast, finds that the small icebergs do not produce striæ, but the large ones, which move over rocks impossible to be got at, "must" produce them. They "appear" to be amply sufficient to do so. If the smaller bergs cannot striate the sea-bottom, why must the larger ones do so? There is no reason why the smaller bergs should not move as swiftly and exert as much pressure on the sea-bottom as the larger ones. And even supposing that they did not, one would expect that the light bergs would effect on a smaller scale what the heavy ones would do on a larger.

I have no doubt that when Mr. Campbell visited Labrador he expected to find the sea-coast under the water-line striated by means of icebergs, and was probably not a little surprised to find that it actually was not. And I have no doubt that were the sea-bottom in the tracks of the large icebergs elevated into view, he would find to his surprise that it was free from striations also.

So far as observation is concerned, we have no grounds from what Mr. Campbell witnessed to conclude that icebergs striate the sea-bottom.

The testimony of Dr. Sutherland, who has had opportunities of seeing the effects of icebergs in Arctic regions, leads us to the same conclusion. "Except," he says, "from the evidence afforded by plants and animals at the bottom, we have *no means whatever* to ascertain the effect produced by icebergs upon the rocks*. In the Malegat and Waigat I have seen whole clusters of these floating islands, drawing from 100 to 250 fathoms, moving to and fro with every return and recession of the tides. I looked very earnestly for grooves and scratches left by icebergs and glaciers in the rocks, but always failed to discover any"†.

We shall now see whether river-ice actually produces striations or not. If floating ice under any form can striate rocks, one would expect that it ought to be done by river-ice, seeing that such ice is obliged to follow one narrow definite track.

St. John's River, New Brunswick:—"This river," says Mr. Campbell, "is obstructed by ice during five months of the year.

* Quart. Journ. Geol. Soc. vol. ix. p. 306.

† Journal, vol. i. p. 38.

When the ice goes, there is wild work on the bank. Arrived at St. John, drove to the suspension-bridge. . . . At this spot, if *anywhere in the world*, river-ice ought to produce striation. The whole drainage of a wide basin, and one of the strongest tides in the world, here work continually in one rock-groove; and in winter this water-power is armed with heavy ice. *There are no striæ about the water-line*”*.

River St. Lawrence:—“In winter the power of ice-floats driven by water-power is tremendous. The river freezes and packs ice till the flow of water is obstructed. The rock-pass at Quebec is like the Narrows at St. John’s, Newfoundland, in the frontispiece. The whole pass, about a mile wide, was paved with great broken slabs and round boulders of worn ice as big as small stacks, piled and tossed, and heaped and scattered upon the level water below and frozen solid. . . . This kind of ice does *not produce striation* at the water-margin at Quebec. At Montreal, when the river ‘goes,’ the ice goes with it with a vengeance. . . . The *piers are not yet striated* by river-ice at Montreal. . . . The rocks at the high-water level have *no trace* of glacial striæ. . . . The rock at Ottawa is rubbed by river-ice every spring, and *always in one direction, but it is not striated*. . . . The surfaces are all rubbed smooth, and the edges of broken beds are rounded where exposed to the ice; *but there are no striæ*”†.

When Sir Charles Lyell visited the St. Lawrence in 1842, at Quebec he went along with Colonel Codrington “and searched carefully below the city in the channel of the St. Lawrence, at low water, near the shore, for the signs of glacial action at the precise point where the chief pressure and friction of packed ice are exerted every year,” but found none.

“At the bridge above the Falls of Montmorenci, over which a large quantity of ice passes every year, the gneiss is polished, and kept perfectly free from lichens, but not more so than rocks similarly situated at waterfalls in Scotland. In none of these places were any long straight grooves observable”‡.

The only thing in the shape of modern ice-markings which he seems to have met with in North America was a few straight furrows $\frac{1}{2}$ an inch broad in soft sandstone, at the base of a cliff at Cape Blomidon in the Bay of Fundy, at a place where during the preceding winter “packed” ice 15 feet thick had been pushed along when the tide rose over the sandstone ledges§.

The very fact that a geologist so eminent as Sir Charles Lyell, after having twice visited North America, and searched specially for modern ice-markings, was able to find only two or

* Short American Tramp, pp. 168, 174. † Ibid. pp. 239–241.

‡ Travels in North America, vol. ii, p. 137.

§ Ibid. vol. ii. p. 174.

three scratches, upon a soft sandstone rock, which he could reasonably attribute to floating ice, ought to have aroused the suspicion of the advocates of the iceberg theory that they had really formed too extravagant notions regarding the potency of floating ice as a striating agent.

There is no reason to believe that the grooves and markings noticed by M. Weibye and others on the Scandinavian coast and other parts of northern Europe were made by icebergs. Mr. Geikie has clearly shown, from the character and direction of the markings, that they are the production of land-ice*. If the floating ice of the St. Lawrence and the icebergs of Labrador are unable to striate and groove the rocks, it is not likely that those of northern Europe will be able to do so.

It will not do for the advocates of the iceberg theory to assume, as they have hitherto done, that, as a matter of course, the sea-bottom is being striated and grooved by means of icebergs. They must prove that. They must either show that, as a matter of fact, icebergs are actually efficient agents in striating the sea-bottom, or prove from mechanical principles that they must be so. The question must be settled either by observation or by reason; mere opinion will not do.

The transporting of boulders and rubbish, and not the grinding and striating of rocks, is evidently the proper function of the iceberg. But even in this respect I fear too much has been attributed to it.

In reading the details of voyages in the Arctic regions one cannot help feeling surprised how seldom reference is made to stones and rubbish being seen on icebergs. Arctic voyagers, like other people when they are alluding to the geological effects of icebergs, speak of enormous quantities of stones being transported by them; but in reading the details of their voyages, the impression conveyed is that icebergs with stones and blocks of rock upon them are the exceptions. The greater portion of the narratives of voyages in Arctic regions consists of interesting and detailed accounts of the voyagers' adventures among the ice. The general appearance of the icebergs, their shape, their size, their height, their colour are all noticed; but rarely is mention made of stones being seen. That the greater number of icebergs have no stones or rubbish on them is borne out by the positive evidence of geologists who have had opportunities of seeing icebergs.

Mr. Campbell says:—"It is remarkable that up to this time we have only seen a few doubtful stones on bergs which we have passed. . . . Though no bergs with stones *on them or in them*

* Proceedings of the Royal Society of Edinburgh, Session 1865-66, page 537.

have been approached during this voyage, many on board the 'Ariel' have been close to bergs heavily laden. A man who has had some experience of ice has *never seen a stone on a berg* in these latitudes. Captain Anderson of the 'Europa,' who is a geologist, has *never seen a stone on a berg* in crossing the Atlantic. *No stones were clearly seen on this trip*"*. Captain Sir James Anderson (who has long been familiar with geology, has spent a considerable part of his life on the Atlantic, and has been accustomed to view the iceberg as a geologist as well as a seaman) has never seen a stone on an iceberg in the Atlantic. This is rather a significant fact.

Sir Charles Lyell states that, when passing icebergs on the Atlantic, he "was most anxious to ascertain whether there was any mud, stones, or fragments of rocks on any one of these floating masses; but after examining about forty of them without perceiving any signs of frozen matter, I left the deck when it was growing dusk"†. After he had gone below, one was said to be seen with something like stones upon it. The Captain and officers of the ship assured him that they had *never seen a stone upon a berg*.

There is still another point connected with icebergs to which we must allude, viz. the opinion that great masses of the boulder-clay of the glacial epoch was formed from the droppings of icebergs. It is perfectly obvious that *unstratified* boulder-clay could not have been formed in this way. Stones, gravel, sand, clay, and mud, the ingredients of boulder-clay, tumbled all together from the back of an iceberg, could not sink to the bottom of the sea without separating. The stones would reach the bottom first, then the gravel, then the sand, then the clay, and last of all the mud, and the whole would settle down in a stratified form. But, besides, how could the *clay* be derived from icebergs? Icebergs derive their materials from the land before they are launched into the deep, and while they are in the form of land-ice. The materials which are found on the backs of icebergs are what fell upon the ice from mountain tops and crags projecting above the ice. Icebergs are chiefly derived from continental ice, such as that of Greenland, where the whole country is buried under one continuous mass, with only a lofty mountain peak here and there rising above the surface. And this is no doubt the chief reason why so few icebergs have stones upon their backs. The continental ice of Greenland is not, like the glaciers of the Alps, covered with loose stones. It is perfectly plain that clay does not fall upon the ice. What falls upon the ice is stones, blocks of rocks, and the loose débris. Clay and

* Short American Tramp, pp. 77, 81, 111.

† Second visit, vol. ii. p. 367.

mud we know, from the accounts given by arctic voyagers, are sometimes washed down upon the coast-ice; but certainly very little of either can possibly get upon an iceberg. Arctic voyagers sometimes speak of seeing clay and mud upon bergs; but it is probable that if they had been near enough they would have found that what they took for clay and mud was merely dust and rubbish.

Undoubtedly the boulder-clay of many places bears unmistakeable evidence of having been formed under water; but it does not on that account follow that it was formed from the droppings of icebergs. The fact that the boulder-clay in every case *is chiefly composed of materials derived from the country on which the clay lies*, proves that it was not formed from matter transported by icebergs. The clay no doubt contains stones and boulders belonging to other countries, which evidently have been transported by icebergs; but the clay itself has not come from another country. But if the clay itself has been derived from the country on which it lies, then it is absurd to suppose that it was deposited from icebergs. The clay and materials which are found on icebergs are derived from the land on which the iceberg is formed; but to suppose that icebergs, after floating about upon the ocean, should always return to the country which gave them birth and deposit their loads is rather an extravagant supposition. And if they did not do that, then how could the boulder-clay always be derived from the country in which it is found?

Boulder-clay, whether formed on dry land or under water, is evidently the product of land-ice. Boulder-clay will be formed under land-ice whether the ice be moving on dry land or along the seabottom. In many parts of England, as at Norwich for example, the boulder-clay contains huge blocks which have evidently come from Scandinavia and other distant parts on the back of icebergs. These have been dropped among the clay then being brought down under the land-ice. In fact during the glacial epoch, unless the seas around a country were completely blocked up by land-ice, it would be impossible to prevent icebergs from coming and discharging their loads occasionally amongst the clay forced from under the land-ice. Few Scandinavian or other far transported blocks are found in the lower boulder-clay of Scotland; and this is owing, no doubt, to the fact that the seas surrounding that country during the glacial epoch were so completely blocked up by land-ice that no berg could possibly approach near to the coast.

If we simply admit (what follows from theory) that as the ice began to accumulate on the land the sea began to rise, and that the time of the greatest extension of the ice was also the time of the greatest submergence, many a difficulty will disappear.

The view which at one time prevailed in regard to this point was, that at the period of continental ice the land stood much higher than at present, and that as the cold began to abate, the land began slowly to sink and went down under the sea covered with ice. The land, after remaining for a long course of ages under water, ultimately emerged still covered with ice; but, the cold continuing to abate, the general covering of ice began to break up, and then followed the period of local glaciers.

I have never been able to find any proof given, either geological or physical, that at the time that the country was enveloped in ice the land stood higher. Instead of a greater elevation of the land during the period of ice, geological facts seem to prove the reverse; for in every country where we have evidence of glaciation, we have also evidence of a corresponding submergence of the land. The two so constantly accompany each other, that many geologists have been led to suspect the existence of some physical bond of connexion between them. We have good evidence, no doubt, that the land in relation to the sea must have stood much higher as well as much lower than it does at present, but we have no evidence that the country was at that particular time covered with ice. On the contrary, geological facts go rather to show that the land was then covered with forests and peat-bogs, and possessed an abundance of animal life.

It follows from theory, as has been shown on former occasions*, that during the cold periods of the glacial epoch the sea must have stood much higher than at present, and during the warm periods much lower. When certain considerations are taken into account which were overlooked on former occasions, a submergence to the extent of at least 500 feet may be accounted for. Although the repeated submergence and emergence of the land during Posttertiary times were probably due to oscillations of the sea rather than of the land, still we have evidence that there must have been local elevations and depressions of the land during the glacial epoch; for we find in some places that the land was submerged to the extent of upwards of 1000 feet, while in other parts it remained above water altogether. The oscillations of the sea-level resulting from cosmical causes would not in any way interfere with upheavals and depressions of the land resulting from volcanic or other agencies.

It was evidently not the facts of geology, but the notion that the cold of the glacial epoch must have resulted from a greater elevation of the land, which gave rise to the opinion that the country stood much higher than at present at the time that it was covered with ice. In order to account for the cold of the

* Phil. Mag. April 1866. 'Reader,' September 2 & 14, 1865; November 27, 1865.

glacial epoch and the general covering of ice, the land was first elevated; and then to make matters harmonize with the facts of geology, the land was submerged.

There is no necessity for this hypothesis in order to account for the cold of the glacial epoch; and even supposing that there was, it would not suit. The cold and ice of the glacial epoch were too general to be accounted for upon the hypothesis of elevation of the land. It is the elevation of a part *in relation to the surrounding* continents that can cool the climate of that part and cover it with ice. A general elevation would produce but little effect; for in such a case the atmosphere would to a considerable extent rise along with the land, and no very sensible change would result. We shall have to return to this part of the subject on a future occasion.

From the facts and considerations adduced we are, I would venture to presume, warranted to conclude that, with the exception of what may have been produced by land-ice, very little in the shape of boulder-clay or striated rocks belonging to the glacial epoch lie buried under the ocean—and that when the now existing land-surfaces are all denuded, probably scarcely a trace of the glacial epoch will then be found, except the huge blocks that were transported by icebergs and dropped into the sea. It is therefore probable that we have as much evidence of the existence of a glacial epoch during the Miocene, Eocene, and Permian periods as the geologists of future ages will have of the existence of a glacial epoch during the Posttertiary period, and that consequently we have no warrant whatever to conclude that the glacial epoch was a something unique in the geological history of our globe.

It might be thought that if glacial epochs have been so numerous as Table No. I. represents, we ought to have abundance of palæontological evidence of their existence. I do not know if this necessarily follows. Take the glacial epoch itself, quite a modern affair. Here we do not require to go and search in the bottom of the sea for the evidence of its existence; for we have the surface of the land in almost identically the same state in which it was when the ice left it, with the boulder-clay and all the wreck of the ice lying upon it. But what geologist, with all these materials before him, would be able to find out from palæontological evidence alone that there had even been such an epoch? He might search the whole, but would not be able to find fossil evidence from which he would be warranted to infer that the country had ever been covered with ice. We have evidence in the fossils of the Crag and other deposits of the existence of a colder condition of climate prior to the true glacial period, and in the shell-beds of the Clyde and other places of a cold condition of climate after the

true glacial period. But in regard to the period of the true boulder-clay or till, when the country was enveloped in ice, all is almost, comparatively speaking, a perfect blank so far as palæontology is concerned. "Whatever may be the cause," says Sir Charles Lyell, "the fact is certain that over large areas in Scotland, Ireland, and Wales, I might add throughout the northern hemisphere on both sides of the Atlantic, the stratified drift of the glacial period is very commonly devoid of fossils"*.

In the "flysch" of the Eocenes of the Alps, in which the huge blocks are found which prove the existence of ice-action during that period, few or no fossils have been found. So devoid of organic remains is that formation, that it is only from its position, says Sir Charles, that it is known to belong to the middle or "nummulitic" portion of the great Eocene series. Again, in the conglomerates at Turin, belonging to the Upper-Miocene period, in which the angular blocks of limestone are found which prove that during that period Alpine glaciers reached the sea-level in the latitude of Italy, not a single organic remain has been found. It would seem that an extreme paucity of organic life is a characteristic of a glacial period, which warrants us to conclude that the absence of organic remains in any formation otherwise indicative of a cold climate cannot be regarded as sufficient evidence that that formation does not belong to a cold period.

But if there is a deficiency of direct positive evidence of a general glaciation of the northern hemisphere during the Middle-Eocene, Upper-Miocene, and other periods similar to what we know took place during the Postpliocene period, there is, however, abundance of indirect evidence in favour of it.

Those facts to which I allude that appear to lead indirectly to the conclusion that a general condition of glaciation must have prevailed during the Upper-Miocene and other periods, are, strange to say, the very facts which have all along been adduced as a reason against the possibility of a cold condition of climate at those periods—notwithstanding the positive evidence which we have that the Alps and the Pyrenees† must have possessed enormous glaciers during that period, those of the former reaching the sea-level in the latitude of Italy.

If a cold and glacial condition of climate prevailed at those periods, we may be perfectly certain that a very warm and equable condition of climate must have also prevailed immediately before

* *Antiquity of Man*, p. 268 (third edition).

† For an account of the evidence of a glaciation of the Pyrenees during the Miocene period, see a paper "On the Glacial Phenomena of the Pyrenees," by Mr. P. W. Stuart Menteath, read before the Edinburgh Geological Society, 1867. See also *Bulletin de la Société Ramond*, 1866.

or immediately after those periods, or perhaps both before and after the cold period. This follows as a necessary consequence from the cosmical theory of climate. For it is physically impossible that we can have a cold and Arctic condition of climate on the one hemisphere, resulting from a great increase of excentricity, without at the same time having a warm, equable, if not an almost tropical, condition of climate prevailing on the other hemisphere. Whether a high state of excentricity will produce in our northern hemisphere a cold and glacial condition of climate or a warm and equable condition, depends solely upon the position of the winter solstice in relation to the perihelion at the time. But if the excentricity continues at a high value during a long course of ages, then, owing to the precession of the equinoxes and motion of the perihelion, the conditions of climate on the two hemispheres will be reversed every ten or twelve thousand years. Consequently when we find, as we do in the Upper-Miocene period and Middle-Eocene period, evidence of a cold condition of climate, we may reasonably expect to find also evidence of a much warmer and more equable condition than now prevails. And this is actually what we do find. "The Upper-Miocene flora and fauna of the whole of Central Europe," says Sir Charles Lyell, "afford unmistakeable evidence of a climate approaching that now only experienced in subtropical regions." "In the present state of the globe the island of Madeira presents the nearest approach to such a flora. The proportion of arborescent as compared to the herbaceous plants is very great; and among the former the predominance of evergreens implies an absence of severe winter cold"*.

And in regard to the conglomerates of the Superga, near Turin, in which the erratic blocks are found, "the fauna and flora both of the overlying and underlying rock," says Sir Charles, "have the same subtropical character as that of Miocene date in Switzerland and Central Europe generally." "Hence," he continues, "the hypothesis of the transport of such huge blocks by ice-action has naturally been resorted to most unwillingly; but in the present state of our knowledge it is the only one which appears tenable."

Here is a case which, if found to be general, ought to settle the whole question of geological climate. Here is a bed of conglomerate indicating a cold and arctic condition of things when it was formed, with icebergs floating around the place now occupied by the city of Turin, overlain and underlain conformably by strata indicating a subtropical condition of climate. But this is not all. This warm condition of things was not confined to Central Europe, but extended probably over the entire northern

* 'Principles,' vol. i. p. 200 (10th edition).

hemisphere, even up to the North Pole ; for it has been proved that Greenland at this time must have been free of ice and covered with a luxuriant flora. (See Heer's *Flora Fossilis Arctica*.) The facts are wholly inexplicable on the ordinary theories of climate. The cosmical theory, however, not only explains them, but they follow according to this theory as a necessary consequence ; for if it should actually turn out that there is no such thing as a warm and equable condition of climate somewhere about the time of an ice-period, then the whole theory would have to be given up, because a warm period, according to theory, is just as necessary a result of an increase of excentricity as a cold period.

The occurrence of a warm condition of climate close beside a cold condition is not a mere accidental circumstance which has only been observed during the Upper-Miocene period. For when we go back to the Middle-Eocene period, we find the "flysch," which bears the marks of having been formed during an ice-period, closely associated with the nummulitic strata, indicating a warm condition of climate. "It has always been objected," says Sir Charles Lyell, "to the hypothesis that these huge masses were transported to their present sites by glaciers or floating ice, that the Eocene strata of nummulitic age in Switzerland, as well as in other parts of Europe, contain genera of fossil plants and animals characteristic of a warm climate. It has been particularly remarked by M. Desor that the strata most *nearly associated* with the flysch in the Alps are rich in echinoderms of the *Spatangus* family, which have a decidedly tropical aspect."

Passing back to the Cretaceous period, we find, closely associated with the floating ice in the sea of the White Chalk, fossil evidence of a warm condition of climate. And then, if we go back to the Permian period, we find glaciers reaching the sea-level in the very centre of England, and other indications of an age of ice, as has been clearly proved by Professor Ramsay. But the fossil remains of the Permian period declare emphatically the prevalence of a warm and equable condition of climate also during that age.

Sir Roderick Murchison has done me the honour to refer, in the last edition of the 'Siluria,' page 548, to my views on the occurrence of glacial epochs during past ages. He is opposed to the opinion held by many geologists and expressed in one of my papers, that there probably was ice-action during the Old-Red-Sandstone and other early periods. Supposing that it were proved that there was no ice during the Palæozoic age, this would not affect the cosmical theory in the least degree ; for it is quite possible, as has been already shown*, that the direct heat of the

* Phil. Mag. S. 4. vol. xxxv. p. 373.

sun may have been greater during the earlier periods of the earth's history. It must also be borne in mind that the simple fact of the fauna and flora of that age indicating a warm and equable condition of climate is not a sufficient proof that there were no cold periods; for a warm and equable condition of climate is just as necessary a result of an increase of excentricity as a cold condition; and the warm periods may be represented by organic remains, and the cold periods not.

Returning to the Postpliocene period. If this period afforded no geological evidence of a warmer condition of climate in Europe than now prevails, it would be so far a presumptive evidence against the assumption that the glacial epoch resulted from cosmical causes. But we have undoubted evidence that the climate of Europe during a portion of that period was much warmer and more equable than at present; for we find evidence of animals and shells existing in latitudes where they could not now live in consequence of the cold. The *Cyrena fluminalis* is a shell which does not live at present in any European river, but inhabits the Nile and parts of Asia, and especially of Cashmere. The *Unio littoralis*, extinct in Britain, is still abundant in the Loire. The *Paludina marginata* does not exist in this country, but inhabits the more southern parts of Europe. These shells have been found in Posttertiary deposits at Gray's Thurrock in Essex, in the valley of the Ouse near Bedford, at Hoxne in Suffolk, in the fluviomarine beds of the Norfolk cliffs, in the freshwater formation at Mundesley, and other parts of England. Along with these shells of a southern type have been found the bones of the Hippopotamus, of a species closely allied to that now inhabiting the Nile, and the *Elephas antiquus*, an animal remarkable also for its southern range, and the *Rhinoceros merghinus*. But what is most remarkable, along with these have been found such animals of an arctic type as the Mammoth, the woolly Rhinoceros, and the Reindeer. How these could all have lived under the same conditions of climate has long been a puzzle to geologists. But many geologists now, as Sir Charles Lyell* and Mr. Boyd Dawkins†, are inclined to believe that these did not all live under the same conditions of climate, but imply oscillations of climate. During a cold condition of climate the Mammoth and other arctic animals would move southwards; and when the climate assumed a warm and equable condition, the animals and shells of a southern type would migrate northwards.

This warm condition of climate was not confined to temperate regions, but extended to high northern latitudes; for we find all over the Arctic regions remains of ancient forests where now the

* 'Principles,' vol. i. p. 193 (tenth edition).

† Quart. Journ. Geol. Soc. May 1867, p. 104.

entire country is covered with snow and ice. The trunk of a white spruce tree was dug up by Sir E. Belcher near Wellington Sound, in lat. $75^{\circ} 32'$ N. The remains of an ancient forest were discovered by Captain M'Clure in Bank's Land in lat. $74^{\circ} 48'$. "This remarkable phenomenon," says Captain M'Clure, "opens a vast field for conjecture; and the imagination becomes bewildered in trying to realize that period in the world's history, when the absence of ice and a milder climate allowed forest-trees to grow in a region where now the ground-willow and dwarf-birch have to struggle for existence." Evidence of ancient forests was found in Prince Patrick's Island and in Melville Island, one of the coldest spots, perhaps, in the northern hemisphere.

These phenomena have excited a very great amount of astonishment, and have been considered very puzzling; but they need not be so regarded if it be the case that the climate of the globe is greatly affected by variations in the excentricity of its orbit; for in such a case there could not possibly be a glacial epoch extending over a long course of time without the frequent recurrence of warm periods. From geological evidence alone, independently of cosmical considerations, we are warranted by analogy from the Posttertiary period to expect the existence of a glacial condition of things during the Upper-Miocene and Middle-Eocene period, from the fact that a warm condition of climate prevailed in Europe and extended to high latitudes during a part of those periods, similar to what we know occurred in the Posttertiary period.

The occurrence of a warm condition of climate always along with a cold condition shows that the two are in some way or other physically connected, and that they are both related to a common cause.

At one time I was under the impression that when the excentricity was near to its superior limit and the winter solstice in the perihelion, the mean annual temperature would not in the temperate regions be higher than it is at the present day, although in all probability at that time the winters would be as warm as the summers, the effect of this condition of things being to produce what Sir John Herschel has called a "perpetual spring." But after making calculations regarding the amount of energy in the form of heat that is being continually transferred from the tropical regions to the temperate and polar regions by ocean-currents, and to what extent the increase in the volume of those currents, which would result from that condition of things, would affect the climate, I felt persuaded that that opinion must be abandoned, and that a "perpetual summer" would better represent the condition of climate which must then prevail.

In the discussion of the subject of geological climate another point has been greatly overlooked, viz. the enormous influence that a mantle of ice continually lying on the continent of Greenland has upon the climate of the northern hemisphere. If we could by some means or other remove this cold mantle off that continent, though all things else should remain the same, the effect that its simple removal would have on the climate of the entire northern hemisphere would be quite magical. On a former occasion we referred to the physical reason of this, but shall have occasion to return again to this part of the question.

In looking over Table I., which, as has already been remarked, probably embraces the greater portion of the Tertiary age, the excentricity several times attained pretty high values, although it is only at the three periods embraced in Tables II., III., and IV. that the values reached would be likely to lead to a condition of things during the past three millions of years that might be properly designated a glacial epoch. But as the excentricity during the greater part of that vast period was greater than it is at the present day (for we are just now much nearer to the inferior limit than we are to the superior limit), its effect on climate would be more marked than it is at present. The effect would be that, as a general rule, during the greater portion of the three millions of years past, the climate would be somewhat colder when the winters occurred in the aphelion than it is at the present day, and a good deal warmer when they occurred in the perihelion than at present.

Judging from geological evidence alone, we naturally conclude that, as a general rule, the climate of the Tertiary period was somewhat warmer than it is at the present day. It is from fossil remains that the geologist principally forms his estimate of the character of the climate during any period. Now, in regard to fossil remains, the warm periods will always be far better represented than the cold; for we find that, as a *general rule, those formations which geologists are inclined to believe indicate a cold condition of climate are remarkably devoid of fossil remains*. If a geologist does not keep this principle in view, he will be very apt to form a wrong estimate of the general character of the climate of a period of such enormous length as the Tertiary.

Suppose that the presently existing sea-bottoms, which have been forming since the commencement of the glacial epoch, were to become consolidated into rock and then elevated into dry land, we should then have a formation which might be properly designated the Postpliocene. It would represent the time which has elapsed from the beginning of the glacial epoch to the present day. Suppose one to be called upon as a geologist to determine from that formation what was the general character

of the climate during the period in question, what would probably be the conclusion at which he would arrive? He would probably find here and there patches of boulder-clay containing striated and ice-worn stones. Now and again he would meet with bones of the mammoth and the reindeer, and shells of an Arctic type. He would likewise find huge blocks of the older rocks imbedded in the formation, from which he would infer the existence of icebergs and glaciers reaching the sea-level. But, on the whole, he would find that the greater portion of the fossil remains met with in this formation implied a warm and temperate condition of climate. At the lower part of the formation, corresponding to the time of the true boulder-clay, there would be such a scarcity of organic remains that he would probably feel at a loss to say whether the climate at that time was cold or hot. But if the intense cold of the glacial epoch was not continuous, but was broken up by one or more warm periods*, during which the ice, to a considerable extent at least, disappeared for a long period of time (and there are few geologists who have properly studied the subject that will positively deny that such was the case), then the country would no doubt during those warm periods possess an abundance of plant and animal life. It is very probable that it was during those periods that the Arctic forests flourished. It is quite true that we may almost search in vain on the present land-surface for the organic remains which belonged to those interglacial periods; for they were nearly all swept away by the ice which followed. But no doubt in the deep recesses of the ocean, buried under hundreds of feet of sand, mud, clay, and gravel, lie multitudes of the plants and animals which then flourished on the land, and were carried down by rivers into the sea. And along with these lie the shells and other marine faunæ which flourished in the warm seas of those periods. A geologist, thus judging from the great abundance of organic remains that this lower portion of the formation would contain indicating a warm condition of climate, and the almost total

* For geological evidence of warm periods during the glacial epoch, see:—Mr. Geikie's memoir "On the Glacial Drift of Scotland;" M. Morlot "On the Posttertiary and Quaternary Formations of Switzerland," Edin. New Phil. Journ., New Series, vol. ii. 1855; Lyell's 'Antiquity of Man,' p. 321, second edition; 'Principles,' vol. i. p. 198, tenth edition; Heer, *Urwelt der Schweiz*; Vogt's 'Lectures on Man,' pp. 318–321; Mr. Edward Hull "On the Drift-deposits in the Neighbourhood of Manchester," Mem. Lit. and Phil. Soc. of Manchester, 1863. For some remarkable facts bearing on the subject, see a valuable memoir by Mr. James Bennie of Glasgow, "On the Surface Geology of the District round Glasgow," Trans. of the Geol. Soc. of Glasgow, vol. iii. part 1. See also a paper by Mr. James Geikie "On the Remains of the *Bos primigenius* found in an Interglacial Bed," Geol. Mag. for September 1868.

absence of fossil remains corresponding to the glacial condition of things which we now know also did prevail at that epoch, would very likely come to the conclusion that the former part of the Postpliocene period was a warm period; whereas we, at the present day, looking at the matter from a different standpoint, declare that part to be a glacial epoch. No doubt, if the beds formed during the cold periods of the glacial epoch could be distinguished from those formed during the warm periods, the fossil remains of the one would indicate a cold condition of climate, and that of the other a warm condition; but still, taking the entire epoch as a whole, the percentage of fossil remains indicative of a warm condition would probably so much exceed that indicative of a cold condition, that we should come to the conclusion that the character of the climate, as a whole, during the epoch in question was warm and equable.

As geologists we have, as a rule, no means of arriving at a knowledge of the character of the climate of any given period but through an examination of the sea-bottoms belonging to that period; for these contain all the evidence upon the subject. But unless we exercise caution, we shall be very apt, in judging of the climate of such a period, to fall into the same error that we have just now seen one would naturally fall into were he called upon to determine the character of the climate during the glacial epoch from the character of the organic remains which lie buried in our adjoining seas.

In conclusion, during the past three millions of years there were three periods when the excentricity attained a high value. These three periods are represented in Tables II., III., and IV. The first began about 2,630,000 years ago, and terminated about 2,460,000 years ago. The second began about 980,000 years ago, and terminated about 720,000 years ago. The third began about 240,000 years ago, and terminated about 80,000 years ago.

The third period, for reasons which have now been considered at some length, I believe to have been that of the Glacial Epoch; the second in all probability was that of the Upper-Miocene period; and the first might probably correspond to the glacial epoch of the Middle-Eocene period.

From the commencement of the glacial epoch, 240,000 years ago, back to the close of the glacial epoch of the Upper-Miocene period, represented in Table III., there is an interval of 480,000 years. Taking the ordinary rate of subaërial denudation, as indicated by the rate at which rivers carry the soil off the land, which we have already seen is not less than about one foot in 6000 years*, it follows that about 80 feet must have been removed off the face of the country during that long interval. It is there-

* *Phil. Mag.* for May 1868, p. 379. *Trans. of Glasgow Geol. Soc.* vol. iii. part. 1.

fore probable that almost every trace of the ice of the Miocene period would be removed before the glacial epoch began. From the close of the glacial epoch of the Middle-Eocene period, represented in Table II., to the commencement of the glacial epoch of the Miocene period, there is the enormous interval of 1,480,000 years. During that time no less than 247 feet would be removed off the general level of the country.

From the close of the glacial epoch of the Miocene period to the present day, 120 feet of rock must have been removed from the surface of the land and carried down in the form of sediment into the sea. And since the glacial epoch of the Eocene period, no less than 410 feet must have been removed. We need not, therefore, wonder that so few traces of the ice of those periods remain. Remove 410 feet of rock off the surface of the present existing continents, and where should we then find our "roches moutonnées," striated rocks, boulder-clay, or in fact any evidence whatever on the land that there had been a glacial epoch during the Posttertiary period?

XLVIII. *Note on the Solvibility of Equations by means of Radicals.* By Professor CAYLEY, F.R.S.*

IN regard to the theorem that the general quintic equation of the n th order is not solvable by radicals, I believe that the proofs which have been given depend, or at any rate that a proof may be given that shall depend, on the following two lemmas:—

I. A one-valued (or symmetrical) function of n letters is a perfect k th power, only when the k th root is a one-valued function of the n letters.

There is an exception in the case $k=2$, whatever be the value of n : viz. the product of the squares of the differences is a one-valued function, a perfect square; but its square root, or the product of the simple differences, is a two-valued function. It is in virtue of this exception that a quadric equation is solvable by radicals; we have the one-valued function $(x_1 - x_2)^2$, the square of a two-valued function $x_1 - x_2$, and thence the two roots are each expressible in the form

$$\frac{1}{2} \{x_1 + x_2 + \sqrt{(x_1 - x_2)^2}\}.$$

II. A two-valued function of n letters is a perfect k th power, only when the k th root is a two-valued function of the n letters.

There is an exception in the case $k=3$, when $n=3$ or 4: viz. for $n=3$ we have $(x_1 + \omega x_2 + \omega^2 x_3)^3$ (ω an imaginary cube root of unity) a two-valued function, and a perfect cube; whereas its cube root is the six-valued function $x_1 + \omega x_2 + \omega^2 x_3$. And similarly for $n=4$ we have, for instance,

$$\{x_1 x_2 + x_3 x_4 + \omega(x_1 x_3 + x_2 x_4) + \omega^2(x_1 x_4 + x_2 x_3)\}^3$$

a two-valued function, and a perfect cube, whereas its cube root

* Communicated by the Author.

is a six-valued function. And it is in virtue of this exception that a cubic or a quartic equation is solvable by radicals. But I assume that for $n > 4$ the lemma is true without exception.

The course of demonstration would be something as follows:—Imagine, if possible, the root of an equation expressed, by means of radicals, in terms of the coefficients; the expression cannot contain any radical such as $\sqrt[p]{X}$, $p > 2$, where X is a one-valued (or rational) function of the coefficients, not a perfect p th power, for the reason that, expressing the coefficients in terms of the roots, such function $\sqrt[p]{X}$ is not a rational function of the roots; if it were so, by lemma I. it would be a one-valued (that is, a symmetrical) function of the roots; consequently a rational function of the coefficients, or X expressed in terms of the coefficients, would be a perfect p th power.

The expression may however contain a radical \sqrt{X} , X a one-valued (or rational) function of the coefficients, not a perfect square; viz. X may be any square function multiplied into that function of the coefficients which is equal to the product of the squared differences of the roots, or, say, multiplied into the discriminant $X = Q^2\Delta$, or $\sqrt{X} = Q\sqrt{\Delta}$.

We have next to consider whether the expression can contain any radical $\sqrt[p]{X}$, where X , not being a rational function of the coefficients, is a function expressible by radicals. But the foregoing reasoning shows that if this be so, X cannot contain any radical other than the radical $\sqrt{Q^2\Delta}$ or $Q\sqrt{\Delta}$, as above; that is, X must be $= P + Q\sqrt{\Delta}$, where P and Q are rational functions of the coefficients, and where we may assume that $P + Q\sqrt{\Delta}$ is not a perfect p th power of a function of the like form $P' + Q'\sqrt{\Delta}$. But then, expressing the coefficients in terms of the roots, we have $P + Q\sqrt{\Delta}$, a (rational) two-valued function of the roots; and there is no radical $\sqrt[p]{P + Q\sqrt{\Delta}}$, which is a rational function of the roots; for by lemma II., if such radical existed we should have $\sqrt[p]{P + Q\sqrt{\Delta}}$, a (rational) two-valued function of the roots; that is, it would be $= P' + Q'\sqrt{\Delta}$, P' and Q' one-valued (symmetrical) functions of the roots, consequently rational functions of the coefficients; or $P + Q\sqrt{\Delta}$ would be a perfect p th power $(P' + Q'\sqrt{\Delta})^p$.

The conclusion is that for $n > 4$ there is not (besides the function $P + Q\sqrt{\Delta}$) any function of the coefficients, expressible by means of radicals, which, when the coefficients are expressed in terms of the roots, will be a rational function of the roots, and consequently no possibility of expressing the roots in terms of the coefficients by means of radicals.

Cambridge, October 1, 1868.

XLIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 317.]

June 18, 1868.—Lieut.-General Sabine, President, in the Chair.

THE following communications were read :—

“An attempt to apply Chemical Principles in explanation of the Action of Poisons.” By W. H. Broadbent, M.D.

The starting-point in the inquiry has been the two following postulates :—

1. That there must be some relation between the substance administered and the animal organism, on which the effects depend.

2. That, so far as the substance is concerned, the basis of the relation can only be its chemical properties, using the term in its widest sense.

From these postulates follow certain corollaries :—

1. That the physiological and therapeutic action of the same substance must be similar in kind.

2. That the action of food, remedies, and poisons must be capable of explanation on the same principles.

3. That substances chemically allied should have similar physiological and therapeutic actions, or any diversity found to exist should be capable of explanation on chemical grounds.

The second of these deductions is taken as a guide in the present inquiry. Something is known as to the uses of the various classes of foods in the economy, and of the mode in which they subserve these uses; this knowledge may be applied in the endeavour to ascertain the mode of action of poisons.

The operations taking place in the animal organism may be divided into two great classes :—(α) for maintenance of structure, (β) for evolution of force. While mutually interdependent, they are distinct, and in character essentially antagonistic—structural and chemical elaboration on the one hand, oxidation or disintegration on the other.

The two great classes of food, organic and mineral, are in close relation with these. The organic foods build up the tissues, but ultimately undergo oxidation and yield force. The inorganic foods take a subordinate part in the composition of the textures; they do not yield force by oxidation, but they influence the nutritive processes. So also the organic remedies and poisons affect the evolution of force, mineral substances the organic processes.

(The action of mineral matters has been noticed elsewhere.)

The force evolved in the animal organism takes the form of heat, motion, and nervous action; but there are very important points of difference between heat on the one hand and nervo-muscular action on the other, both as to the part they take in the vital processes, and in the conditions of their evolution.

It is through their action on the nervous system that the powerful organic poisons destroy life; and in order to understand this

action, it is necessary to consider closely the evolution of nerve-force, and to endeavour to realize the chemical conditions implied.

In the first place, the source of nerve-force is oxidation, and the seat of the oxidation is the nervous structures. This is generally admitted, and seems to be conclusively established by an analysis of the phenomena observed in experiments with a prepared frog's leg.

This admitted, it is to be noted—

1. That nerve-action is intermittent and of varying intensity, and that, in addition to the presence of the oxygen brought to the nerve-structures by the blood, an impulse from without, or from some other part of the nervous system, is necessary to determine the evolution of the force.

2. Again, there is a storing up of potential energy in the nervous structures; witness the necessity for sleep, &c.

3. A due supply of oxygen is required. The phenomena of asphyxia show that the demand is most urgent in the hemispherical ganglia.

These facts indicate that the constituent of the nervous structures by oxidation of which the force is yielded, possesses what I have ventured to call chemical tension, a property which does not belong to non-nitrogenized matter, or to all nitrogenized matter. It will be further explained later; for the present, it is sufficient to refer to nitroglycerine as an extreme example.

The protagon of Liebreich, and the neurine recently identified by Wurtz with hydrate of trimethyl-oxethyl-ammonium, have this characteristic in a certain degree.

Turning now to the poisons which kill by their powerful action on the nervous system. They all contain nitrogen, and all possess chemical tension; and these seem to be the only points common to the entire group.

Nitrogen cannot be the poisonous element; it has no great chemical energy, and it is present in large proportion in substances which are inert. It is nevertheless the pivot on which the deadly influence turns. Its affinity for H, O, and especially for C, is only feeble. When, therefore, in a molecule containing C, H, and N, or C, H, N, and O, the elements are not so arranged that the mutual affinities of C, H, and O cooperate to maintain the integrity of the molecule; there may be a more or less powerful tendency on the part of C, H, and O to rearrange themselves without regard to the N, or to combine with O or H_2O if presented. This is what is meant by the term chemical tension. In the example given, nitroglycerine $\{\text{C}_3\text{H}_5(\text{NO}_2)_3\text{O}_3\}$, the dislocation is of O from N in favour of C and H. Equally striking examples of dislocation of N from C or H cannot be given, and it is not easy in all cases to point out the source of the tension. A very important method in which the balance of affinities is deranged and the condition of tension brought about, is by departure from a stable type, as, for instance, in the nitrite bases which are residues derived from ammonium-salts by dehydration. To this class belong morphia, strychnia, brucia, and most poisonous alkaloids. Additional interest is given to these compounds by the fact that Dr. Crum Brown and Dr. Fraser have shown that, by in-

troduction into the molecule of methyl-iodide, carrying back the constitution a step in the direction of the ammonium type, the poisonous effects are greatly diminished, and entirely altered in character.

Let the deduction as to the evolution of nerve-force be accepted, and we have in the introduction into the blood of substances having varying degrees and directions of tension an intelligible method of influencing its manifestations.

Looking now upon nerve-action as a result of oxidation, in the various methods by which this oxidation may be influenced, analogies may be traced with conditions which affect ordinary combustion. These conditions are :—

1. The supply of oxygen.
2. The character of the combustible.
3. The presence of products of combustion, or of bodies having a similar influence.

It is of course necessary to bear in mind the peculiarities of the oxidation yielding nerve-force, the differences between combustion and oxidation in the moist state, and the special modifying conditions of the animal organism. For example, while in asphyxia the deprivation of oxygen arrests all nervous action, the respiration of undiluted oxygen does not intensify it, either because the blood will only take up a certain proportion of oxygen, or more probably because the effects of the O are expended in altering the blood, which is thus oxidized instead of being oxygenated.

The analogies to the above conditions found in the action of substances on the nervous system are :—

1. The liberation in the nascent state in the nervous structures of C and H, which appropriate the O brought by the blood, and so produce a result equivalent to the exclusion of O. The C and H are set free by the dislocating influence of N; and the example of this mode of action is furnished by prussic acid.

The converse of this, the liberation of O by a similar process, is not likely to occur, as O is never present in an organic body in excess of the proportion which would fully oxidize the other elementary constituents.

2. The analogy to the influence on the energy of combustion by the character of the combustible, is found in the introduction into the blood of substances having chemical tension, holding different relations to the tension of the nervous matter.

3. The action of anæsthetics on the nervous system furnishes a strict parallel to the influence of CO_2 on combustion.

The rationale here given as to the action of anæsthetics is, for the purposes of the present paper, taken as established by the late Dr. Snow. Objections which have been made to it are capable of removal by experiments and considerations which need not here be adduced.

Considerable importance is attached to the establishment of the explanation here given of the action of prussic acid. Stated more explicitly, this explanation is, that the prussic acid is carried by the blood to the nerve-centres, that, under the influence of the affi-

nities thus brought to bear upon it (affinities which normally determine the oxidation by which nerve-force is evolved), its elements are dislocated from each other; and the C and H, liberated in the latent condition, appropriate the O destined for the evolution of nerve-force, which is thus arrested.

This explanation is suggested by the composition of hydrocyanic acid, H Cy. Cyanide of potassium KCy, again, is used as a powerful reducing agent in chemical processes. Its liability to the change which will permit its elements to exercise their individual affinity for O is indicated by its spontaneous decomposition in water, by its position as a nitrile (formio-nitrile). Corroborative evidence that it is by means of such a change that it acts, is furnished when the elements are held together by some supporting affinity, as in ferrocyanogen. But the best example is in sulphocyanogen and hydrosulphocyanic acid, which of themselves are poisonous (*i. e.* cannot resist the dislocating influences), but reinforced by a base are innocent. [A parallel to this is seen in aniline, which is poisonous, and in sulphate of aniline, which is not. See Lond. Hosp. Reports, Dr. Letheby.]

The phenomena of poisoning by prussic acid are perfectly consistent with this view. All observers have noted their similarity to those of asphyxia.

Still more striking is the fact that artificial respiration, and especially with oxygen, is the great means of neutralizing the effects of this poison.

Probably this chain of facts would be considered conclusive, were it not that the hypothesis as to the mode of death by H Cy is paralysis of the respiratory movements. This hypothesis, however, still leaves unexplained the cause of the paralysis itself, and therefore the real mode of action of the poison. It is, moreover, inconsistent with certain of the phenomena; the respiratory nerve-centres are actually the last to be paralyzed, except those concerned in the action of the heart.

Experiments, nevertheless, have been made for the purpose of ascertaining whether the previous respiration of undiluted oxygen would retard, or in any measure prevent the action of prussic acid. The results have been by no means uniform; but instances have occurred (rats being the animals used) in which, after the injection of an overwhelming dose, the fatal effect has been delayed quite beyond the operation of accidental causes, and, again, in which a dose fatal to two rats, and barely survived (after a long train of symptoms) by one other, produced comparatively little effect on another after the respiration of oxygen.

It has been found also that a proportion of prussic acid diffused in equal volumes of air and of oxygen, has a decidedly less powerful action on the animal in the latter case.

With frogs the results were most contradictory and embarrassing, till it was discovered that prussic acid injected under the skin had scarcely any action on them; but if they were subsequently placed under a glass shade, or in some other confined atmosphere, into which the acid diffused, it would gradually affect them. It seems

probable that the affinities in operation in the nervous structures of the frog are not sufficiently energetic to determine the decomposition of the H Cy, which will then act upon this animal as an anæsthetic.

Nitroglycerine was at first made the subject of experiment, under the idea that possibly oxygen might be evolved from the NO_2 which is substituted for three equivalents of H in the typical molecule. Subsequent reflection showed that this is not likely to occur; but the fact remains, that it is a substance liable to change, and very highly charged with oxygen, as compared with the ordinary constituents of the body.

It is a very powerful poison, having, however, entirely different effects on frogs and rats.

In frogs it very speedily induces powerful tetanic convulsions: a single drop of a solution of one pint of nitroglycerine in four parts of methyl in alcohol placed on the back of a frog is followed in five or eight minutes by stiffness of movement, and in thirteen to sixteen minutes by most violent spasms. In rats an hour or more elapses before any symptom is manifest, and then death is by a gradually increasing feebleness of movement, in two or three hours, without convulsion.

It is unquestionable that this difference in the effects has a relation to the oxygen contained in the nitroglycerine. The contrast with prussic acid in the action on warm and cold-blooded animals is suggested.

A very extended and comprehensive inquiry, both as to the conditions in the nervous system associated with convulsions, tetanus, delirium, &c., and into the relations and constitution of the poisons which give rise to these symptoms, is necessary before the second analogy can be followed out with any confidence. Experiments are being made with substances of known composition and constitution, with a view to elucidate this part of the question.

In conclusion, two points are considered which cannot be passed over in any attempt to apply the principles of physical and chemical science to the case of poisons.

The first is as to the minuteness of the fatal dose. Any explanation, before it can be accepted, must show that the cause is adequate to produce the effect. This is a difficulty in the path of any rational explanation. It is attempted to meet it by showing, on the one hand, that the equivalency of nerve-force is extremely small, by reference to its analogy with electrical currents, and by other considerations, and that therefore the degree of chemical change involved in its evolution is also small; and, on the other hand, the maximum of force to be obtained from an organic body is through the exercise of the affinities of its individual elements.

The second point is as to the special action of certain poisons on particular nervous centres—strychnia on the cord, morphia on the brain, &c., the substances being carried by the blood to all alike. It is as necessary to explain why no effect is produced on those centres or tracts which do not suffer, as to explain the action on the one which does. The explanation is sought in the fact that the differ-

ence in the functional activity of the brain and cord, the need for sleep by the brain, not experienced, at any rate in the same degree, by the cord, point to a difference of tension, and therefore of relation to the substances which act as poisons. This consideration will apply where the differences of susceptibility and of tension are not so marked.

But this is only part of a still wider question—the different action of poisons on different classes of animals. The explanation is still the same. Difference in the functional energy or activity of corresponding nerve-centres implies difference of tension.

The following facts bear strikingly on this point:—

1. *Analgesics* affect all classes of animals alike, *i. e.* when the effect is a general arrest of oxidation.

2. *Strychnia*, which acts on the cord, affects all animals alike. The spinal system is the centre which is most similar in its endowments in all classes of vertebrates.

3. The poisons which have the most diverse action on different animals are such as in man act on the cerebral ganglia.

“On the Spectrum of Comet II., 1868.” By William Huggins, F.R.S.

The author describes the appearance of the comet in the telescope on June 22 to consist of a nearly circular coma, which became rather suddenly brighter towards the centre, where there was a nearly round spot of light. A tail was traced for nearly a degree.

He found the light of the comet, when examined with a spectro-scope furnished with two prisms of 60°, to be resolved into three broad bright bands.

The brightest band commences at about *b*, and extends nearly to *F*. Another band begins at a distance beyond *F* rather greater than half the interval between *b* and *F*. The third band occurs about midway between *D* and *E*. In the two more refrangible of these bands the light was brightest at the less refrangible end, and gradually diminished towards the other limit of the bands. The least refrangible of the three bands did not exhibit a similar gradation of brightness.

These bands could not be resolved into lines, nor was any light seen beyond the bands towards the violet and the red.

The measures of these bands are given, and a diagram of their appearance.

The author found this cometic spectrum to agree exactly with a form of the spectrum of carbon which he had observed and measured in 1864. When an induction spark, with Leyden jars intercalated, is taken in a current of olefiant gas, the highly heated vapour of carbon exhibits a spectrum which is somewhat modified from that which may be regarded as typical of carbon. The light is of the same refrangibilities, but the separate strong lines are not to be distinguished. The shading (composed of numerous fine lines) which accompanies the lines appears as an unresolved nebulous light.

On June 23 the spectrum of the comet was compared directly

in the spectroscope with the spectrum of the induction spark taken in a current of olefiant gas.

The three bands of the comet appeared to coincide with the corresponding bands of the spectrum of carbon. In addition to an apparent identity of position, the bands in the two spectra were very similar in their general characters and in their relative brightness.

These observations were confirmed on June 25.

The remarkably close resemblance of the spectrum of the comet to that of the spectrum of carbon, necessarily suggests the identity of the substances by which in both cases the light was emitted.

The great fixity of carbon seems, indeed, to raise some difficulty in the way of accepting the apparently obvious inference from these prismatic observations. Some comets have approached sufficiently near the sun to acquire a temperature high enough to convert even carbon into vapour.

In the case of other comets, the author suggests that the difficulty is one of degree only; for the conditions are not known under which even a gas permanent at the temperature of the earth could maintain sufficient heat to emit light.

The author states that some phosphorescent substances give spectra which are discontinuous, but he gives reasons which would scarcely permit us to consider cometary light to be of a phosphorescent character.

The spectrum shows that the colour of this comet was bluish green. Considerable difference of colour has been remarked in the parts of some comets. Sir William Herschel described the head of the comet of 1811 to be of a greenish or bluish-green colour, while the central point appeared of a ruddy tint. The same colours have been observed in other comets. If carbon be the substance of some comets, this substance, if incandescent in the solid state, or reflecting, when in a condition of minute division, the light of the sun, would afford a light which, in comparison with that emitted by the luminous vapour of carbon, would appear yellowish or approaching to red.

The author refers to the bearing of these results on certain cometary phenomena, and on the apparent identity of the orbits of the periodical meteors with those of some comets.

L. Intelligence and Miscellaneous Articles.

ON STELLAR SPECTRA. BY FATHER SECCHI.

IN one of my previous researches on stellar spectra I pointed out that researches on coloured stars led us to admit a fourth type of stellar spectra, the standard of which might be the spectrum which I have published of the star 12561 of Lalande. Now (after having completed a review of the coloured stars) I am absolutely certain of this fact. This type had escaped me in my previous researches; for none of them exceeds the sixth magnitude; and it would not have been remarked without the introduction of the cylindrical eyepiece, which leaves to the spectra of these stars all their brilliancy.

The essential character of this type is to present a spectrum formed of three luminous bands separated by dark intervals. The brightest band is in the green; it is generally strong, well marked, and very dilated. Another, much feebler band is met with in the blue; but this band is often very difficultly visible. The third band is found in the yellow, and expands towards the red; this, however, is divided into several others.

All these bands have the characteristic that their light increases in the direction of the violet, and then suddenly ceases. On the contrary, towards the red they gradually shade into absolute black. There is thus a complete opposition between this type and the third; for in this not only are the columns double in an equal space, but they present the maximum of light on the side of the red, and the minimum on that of the violet. The two spectra are therefore not a modification of one and the same type; they are evidently due to substances completely different.

The feebleness of their light, which prevents us from using the spectroscope with a slit, hinders a rigorous definition of the substances which produce these phenomena. It may, however, be said that there is great analogy with the reversed carbon spectrum. But I think any conclusion of this kind is premature.

The following is a list of the principal stars of this type, and their position:—

Number in Schellerup's catalogue.	Right ascension.	Declination.	Magnitude.	Notes.
	h m s			
41.	4 36 2	+67° 54'	6½	Beautiful.
43.	4 42 8	+28 16	8	
51.	4 58 1	+ 0 59	6	
78.	6 26 9	+38 33	6½	Beautiful.
89.	7 11 5	—11 43	7½	
124.	9 44 6	—22 22	6½	
128.	10 5 8	—34 38	7	Doubtful.
132.	10 30 7	—12 39	6	Beautiful.
136.	10 44 8	—20 30	6½	Magnificent.
152.	12 38 5	+46 13	6	
159.	13 19 3	—11 59	5½	
163.	13 47 3	+41 2	7	
229.	19 26 5	+76 17	6½	
238.	20 8 6	—21 45	6	Beautiful.
249.	21 25 8	+50 58	9	
252.	21 38 6	+37 13	8½	
273.	23 39 2	+ 2 42	6	

A description of these spectra, with figures, will be inserted in the memoir which is ready for publication; this memoir will comprehend the details of the third type, all the spectra of which present a superb colonnade. I will here restrict myself to giving the curve of the light of the 152nd, which is the most beautiful; the projecting lines resemble dazzling and very brilliant lines near the end of the luminous band.

It is to be presumed that among the smaller stars some other spectrum will be found; but for this purpose I think it is indispensable to use a telescope with a greater power than that of nine inches, or a more delicate eye. Nearly all red stars have spectra with bands; the small ones often present a continuous spectrum: this continuity might be merely apparent, and these stars would belong to the red type without bands, as is the case with Arcturus among the large ones. In my researches I have not limited myself to the stars of the catalogue, but each time I have traversed the surrounding heavens; thus I have discovered some new stars, but in small number, which leads me to believe that the greater number have been noted, at any rate for our hemisphere. A similar study would have to be undertaken for the southern hemisphere.

Having at length been able to put up the complete apparatus for the absolute measurement of the lines and their comparison with chemical elements, I have confirmed that the rays of hydrogen coincide with the four black lines of α Lyræ with surprising accuracy. I say the four lines; for even the most refrangible line of the violet is found in its place in the spectrum of hydrogen. The tube with which the observation was made was sent to me by M. Geissler of Bonn, who prepared it with chemically pure hydrogen. The spectrum thus obtained agrees with that which has been given by M. Morren of Marseilles. Another secondary line of hydrogen appeared in the spectrum of the star, like that of the yellow. After this it can no longer be doubted that this substance forms the absorbing atmosphere of this star, which was already known. But it is also evident that the star α Lyræ has no appreciable motion of its own the velocity of which is comparable to that of light, as I have said in another communication. The spectroscope used was made by M. Hoffmann of Paris. It was formed of two very dispersive prisms; and when it was directed towards the sun, it separated the two lines of sodium and the three lines of magnesium with great sharpness.

The coincidence of the lines of hydrogen with those of the light of the sun reflected by the moon presented the same degree of precision and exactitude as for α Lyræ. At the time of observation this star was almost on the zenith. The observation was made on the 2nd and 3rd of this month (August). It remains to be seen if, with this delicate means, we do not succeed in ascertaining the influence of the motion of the earth. I do not think it is impossible to appreciate differences amounting to one-tenth the magnitude of the line *f*. —*Comptes Rendus*, August 10, 1868.

ON A THERMO-RHEOMETER. BY M. JAMIN.

The instrument which I have the honour of submitting to the Academy is a water-thermometer. The reservoir is a long glass tube with a graduated stem which is bent downwards and terminates in a cup in which water can be placed to fill the instrument. The bottom of the reservoir is fastened in a mercury-cistern arranged as in a Fortin's barometer. The mercury can thus be raised or lowered by a measurable amount, which diminishes or increases the quantity of water in the reservoir; and if its temperature be raised without

heating the mercury, the end of the column in the stem moves. The delicacy of the thermometer varies with the height of the mercury, but according to a simple law which is deduced from calculation or from observation.

A very fine platinum wire is stretched in the reservoir from the top to the cistern; its ends are fused into the glass and connected with the poles of a battery; it transmits the current through the mercury without resistance; offering through the water a resistance x , it develops an amount of heat which only affects the water of the thermometer, which does not heat the metal, and which cannot be transmitted downwards owing to the feeble conductivity of the liquid.

(1) By raising or lowering the mercury, the length and resistance x of the platinum wire are diminished or increased at pleasure. The apparatus thus becomes a rheostat analogous to that of Pouillet, but more convenient and better, inasmuch as the wire, being immersed in water, is but little heated, and serves for currents whether strong or weak.

(2) The heat developed by the passage of a current may be measured. It equals $p(t' - t)$, the product of the weight of water into the increase of temperature; the weight is sxd . The increase of temperature is measured by the variation of the volumes, or the number n of divisions through which the thermometer-column has moved, divided by the volume sx , and by the coefficient of expansion k . Then

$$p(t' - t) = \frac{sxd}{sxk} n = \frac{d}{k} n,$$

which signifies that the heat given up, apart from the corrections, which I here neglect, may be measured by the number of divisions by which the thermometer-column has moved; n is independent of the height of the mercury. A simpler result could not be arrived at. The measurement of n is made with the ordinary precautions.

(3) This heat n is proportional to the resistance x , which is determined by the square of the intensity. We have then

$$i = \sqrt{\frac{n}{x}}.$$

The instrument may thus be used as a galvanometer; the smaller the ratio of the sections of the stem and of the reservoir, the more delicate it is. In fact it is a thermo-rheometer.

(4) Replacing i by its value, we get

$$\frac{A^2 x}{(R + x)^2} = n.$$

Making the determinations of n with different values of x , A and R may be calculated; that is, the electromotive force and the resistance of a battery may be determined without the aid of any other instrument than the thermo-rheometer.

(5) What has been said applies to induction-currents as well as to the current of batteries. The former have hitherto not been much investigated: as they are alternately contrary, it is impossible to separate them accurately; and in general their effects neutralize themselves. The calorific effect alone is independent of the direction of the currents and unaffected by their interruptions; and, finally, the

sum of the heats observed by the thermo-rheometer is proportional to the square of the electricity put in circulation.

In short, the thermo-rheometer is at once and of itself a rheostat, a galvanometer, and a measurer of electromotive forces; it is the only one of these various instruments which can be applied to induction-currents as easily as to ordinary currents.—*Comptes Rendus*, July 6, 1868.

INVESTIGATIONS ON OBSCURE CALORIFIC SPECTRA.

BY M. DESAINS.

The whole of the researches relative to the calorific spectrum have long ago established that the heat of the luminous part of this spectrum is more transmissible through water than are the mean obscure rays. Melloni has gone further, and, in a memoir presented in 1831 to the Academy, announced that the least-refrangible parts of the obscure solar spectrum are completely destitute of the property of traversing a layer of water a few millimetres thick. These portions, from this point of view, would resemble the radiations from blackened bodies heated to temperatures below 300° .

Whatever since Melloni's time be the results which science has acquired relative to the properties of solar calorific radiations, I have thought it right to investigate again whether, in the spectra formed of these radiations, there really are rays analogous to those of sources at a very low temperature.

The ideas recently acquired on the absorbing action of vapours made it little probable; and in fact I could never find, in the solar spectra I have investigated, rays completely deprived of transmissibility through water. These spectra were so pure that the eye could readily detect the principal lines; the luminous part was about 25 millims. in extent, and their obscure part about the same. I examined the different parts by the aid of a line pile, the breadth of which was scarcely a twenty-fifth of the whole breadth of the spectrum. In the maximum I had frequently a deviation of 30° , and sometimes much more; but on removing the pile to the limits of the obscure, where there was only a deviation of one or two divisions, I found that the heat capable of producing these deviations was transmitted in considerable proportion through a layer of water of 2 millims. It is not, perhaps, superfluous to remark that these latter pencils of obscure heat did not in my experiments exceed the two-hundredth part of the total heat spread over the whole extent of the spectrum.

Entirely different results are obtained on analyzing the spectra of terrestrial luminous sources, such as the flame of a lamp without a glass, or an incandescent platinum wire.

In one series of experiments a wire of this kind was kept at a cherry-red heat in a small gas-flame. Its radiation, confined by a suitable diaphragm, fell on a rock-salt lens with a focus of 15 centims., which formed a well-defined image at a distance a little greater than 30 centims. With a fine prism of rock-salt, the rays were deviated and dispersed. The luminous portion of the spectrum was barely visible, and the calorific effects it produced of feeble intensity. The dark spectrum was at least as expanded as in the case

of the solar rays, and it exhibited a well-defined maximum; in the position relative to this maximum, the pile received so much heat that the deviation obtained often amounted to 20° . On the dark side the spectrum extended to a distance at least equal to that which separated the maximum from the luminous region. The whole of this second part of the spectrum was completely absorbable by a layer of water 2 millims. in thickness; and the rays forming the maximum were almost completely absorbed. Scarcely a third part of the rays escaped absorption, and it was necessary to approach the luminous region to rediscover well-marked transmissions. Thus in a spectrum formed by rays which have not had to traverse an absorbing layer of appreciable thickness, rays are found which are analogous to those ordinarily proceeding from the sides of heated enclosures.

The apparatus I used in these spectrum-studies enabled me to measure to a quarter of a degree the deviation of the pencil by the prism of rock-salt, which was always placed in the position in which the deviation of the red was a minimum. In making these measurements I soon ascertained that pencils of the same breadth, and the same mean refrangibility, were very differently absorbable by 2 millims. of water, according as I took them in the solar spectrum or in a spectrum formed by rays emanating from platinum. In this latter case the absorption is stronger than in the other.

The differences are very precise, and cannot be attributed to the presence of rays quite absorbable coming from the extremities of the spectrum and which, owing to a defective arrangement of the apparatus, were mixed with the rays to be operated upon. If, in fact, there were such a flux of absorbable rays, there ought to be another of rays much more transmissible coming from the most refracted portion of the spectrum, and the effects would almost balance each other. Moreover the following new experiment could leave no doubt. A moderator lamp was taken as source of heat, and a very pure spectrum was formed with the rays proceeding from it. In one of my experiments the deviation of the red rays was $40^{\circ}5$, that of the violet 43° ; and I obtained a few tenths of deviation by placing the index which supported my pile at a distance of 38° from the direct rays. When the alidade was $39^{\circ}9$ from the incident ray, the pile received no luminous ray, but was covered by a luminous band near the extreme red. A trough of fluor-spar containing a layer of water of 2 millims. thickness allowed about a third of the rays forming this band to pass. Thus, for instance, 33° of deviation were obtained with the direct, and 11° with the transmitted. Placing the index at $39^{\circ}2$ from the direction of the incident rays, strong deflections in the direct were still observed; but the transmissibility through the same trough was singularly diminished; it did not amount to more than $\frac{31}{100}$; that is, the mean deviation for the direct amounted to 19° , and for the transmitted to $3^{\circ}1$.

Having made these observations, I modified the nature of the rays by causing them, before falling on the prism, to pass through a layer of water of 0.003 metre in thickness contained between two glass plates. For the two positions which I have mentioned, the trans-

missibility through the trough of spar filled with water was remarkably increased; it became $\frac{4}{3}$ in the first case, and $\frac{1}{2}\frac{9}{5}$ in the other.

From these numbers a complete demonstration of the propositions previously enunciated appears to follow. In fact, if the trough filled with water only allows a third of the rays near the extreme red to pass, and far less of the other and less refrangible obscure pencils, it is impossible to assume that a mixture of rays so little transmissible could form a pencil capable of being transmitted in the proportion of $\frac{1}{2}\frac{9}{5}$ or $\frac{4}{3}$. Yet this consequence must be admitted if it be allowed that the trough of water only modifies the incident pencils in the degree that it clears them by extinguishing rays of mean refrangibility very different from those of the pencils investigated.

It seems to me made out that if in very fine spectra, but of different sources, we isolate two layers of rays of the same mean refrangibility, and the breadth of which (the same in both cases) is a very small portion of the total breadth of the spectrum, these two layers of rays may have very different properties.

Without discussing here the various interpretations which might be proposed of this fact, I will restrict myself to the remark that it may be completely explained by the data which spectrum-analysis furnishes. In fact, if in a solar spectrum, whether luminous or chemical, we consider a band the breadth of which is a fifteenth to a thirtieth of that of the spectrum, we know that this band is filled with a number of lines which correspond to rays that have been absorbed in passing through the solar or terrestrial atmosphere; and, though with considerable differences, absorbents conveniently chosen may produce analogous effects on spectra from terrestrial sources. Now the lines (the bands of absorption in question) being developed in a pencil of definite mean refrangibility, necessarily modify it, both as regards intensity and transmissibility, since obviously the rays which have disappeared were not the same as those which remained, though they had the same degree of refrangibility.

In concluding, I beg to mention a fact which is directly connected with the study of calorific spectra. A body raised to a red heat emits both dark and luminous calorific rays. If it be heated from the point at which it becomes distinctly luminous, is the increase in the energy of its radiation restricted to the luminous part alone, or does it extend to both at once? The simple fact that the maximum heat is greatest in the dark part of the solar spectrum seems to indicate that the second hypothesis is true. It may moreover be verified in the following manner. We take as source of heat a plate of platinum forming a side of a small gas-furnace, in which gradually higher temperatures may be obtained by increasing the pressure of the air. When the plate is at a distinct cherry-red, part of the rays it emits are isolated and dispersed by a prism. The pile, placed in the obscure part of the spectrum at a considerable distance from the luminous part, remains fixed as long as the pressure of the air injected is the same; if the pressure be increased, the plate passes from cherry to white, and at the same time the thermoscopic indication is greatly increased, although the pile cannot receive any luminous ray.—*Comptes Rendus*, August 3, 1868.

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LI. *On the Communication of Vibration from a Vibrating Body to a surrounding Gas.* By G. G. STOKES, M.A., D.C.L., Sec. R.S., Fellow of Pembroke College, and Lucasian Professor of Mathematics in the University of Cambridge*.

IN the first volume of the Transactions of the Cambridge Philosophical Society is a short paper by Professor John Leslie, "On Sounds excited in Hydrogen Gas," in which the author mentions some remarkable experiments indicating the singular incapacity of hydrogen for becoming the vehicle of the transmission of sound when a bell is struck in that gas, either pure or mixed with air. With reference to the most striking of his experiments, the author observes (p. 267), "The most remarkable fact is, that the admixture of hydrogen gas with atmospheric air has a predominant influence in blunting or stifling sound. If one half of the volume of atmospheric air be extracted [from the receiver of the air-pump], and hydrogen gas be admitted to fill the vacant space, the sound will now become scarcely audible."

No definite explanation of the results is given; but with reference to the feebleness of sound in hydrogen the author observes, "These facts, I think, depend partly on the tenuity of hydrogen gas, and partly on the rapidity with which the pulsations of sound are conveyed through this very elastic medium;" and he states that, according to his view, he "should expect the intensity of sound to be diminished 100 times, or in the compound ratio of its tenuity and of the square of the velocity with which it conveys the vibratory impressions." With reference to the effect of the admixture of hydrogen with air he says, "When

* From the Philosophical Transactions for 1868, Part II., having been read June 18, 1868.

hydrogen gas is mixed with common air, it probably does not intimately combine, but dissipates the pulsatory impressions before the sound is rigorously formed."

In referring to Leslie's experiment in which a half-exhausted receiver is filled up with hydrogen, Sir John Herschel suggests a possible explanation founded on Dalton's hypothesis that every gas acts as a vacuum towards every other*. According to this view there is a constant tendency for sound-waves to be propagated with different velocities in the air and hydrogen of which the mixture consists, but this tendency is constantly checked by the resistance which one gas opposes to the passage of another, calling into play something analogous to internal friction, whereby the sound-vibration, though at first produced, is rapidly stifled. Air itself indeed is a mixture; but the velocities of propagation of sound in nitrogen and oxygen are so nearly equal that the effect is supposed not to be sensible in this case.

This explanation never satisfied me, believing, as I always have done, for reasons which it would take too long here to explain, that for purely hydrodynamical phenomena (such as those of sound) an intimate mixture of gases was equivalent to a single homogeneous medium. I had some idea of repeating the experiment, thinking that possibly Leslie might not have allowed sufficient time for the gases to be perfectly mixed (though that did not appear likely), when another explanation occurred to me, which immediately struck me as being in all probability the true one.

In reading some years ago an investigation of Mr. Earnshaw's, in which a certain result relating to the propagation of sound in a straight tube was expressed in terms, among other things, of the velocity of propagation, the idea occurred to me that the high velocity of propagation of sound in hydrogen would account for the result of Leslie's experiment, though in a manner altogether different from anything relating to the propagation of sound in one dimension only.

Suppose a person to move his hand to and fro through a small space. The motion which is occasioned in the air is almost exactly the same as it would have been if the air had been an incompressible fluid. There is a mere local reciprocating motion, in which the air immediately in front is pushed forwards, and that immediately behind impelled after the moving body, while in the anterior space generally the air recedes from the encroachment of the moving body, and in the posterior space generally flows in from all sides, to supply the vacuum which tends to be created; so that in lateral directions the motion of the fluid is backwards, a portion of the excess of fluid in the front going to

* *Encyclopædia Metropolitana*, vol. iv. art. Sound. § 108.

supply the deficiency behind. Now conceive the periodic time of the motion to be continually diminished. Gradually the alternation of movement becomes too rapid to permit of the full establishment of the merely local reciprocating flow; the air is sensibly compressed and rarefied, and a sensible sound-wave (or wave of the same nature, in case the periodic time be beyond the limits suitable to hearing) is propagated to a distance. The same takes place in any gas; and the more rapid the propagation of condensations and rarefactions in the gas, the more nearly will it approach, in relation to the motions we have under consideration, to the condition of an incompressible fluid; the more nearly will the conditions of the displacement of the gas at the surface of the solid be satisfied by a merely local reciprocating flow.

This explanation, when once it suggested itself, seemed so simple and obvious that I could not doubt that it afforded the true mode of accounting for the phenomenon. It remained only to test the accuracy of the assigned cause by actual numerical calculation in some case or cases sufficiently simple to permit of precise analytical determination. The result of calculations of the kind applied to a sphere proved that the assigned cause was abundantly sufficient to account for the observed result. I have not hitherto published these results; but, as the phenomenon has not to my knowledge been satisfactorily explained by others, I venture to hope that the explanation I have to offer, simple as it is in principle, may not be unworthy of the notice of the Royal Society.

For the purpose of exact analytical investigation I have taken the two cases of a vibrating sphere and a long vibrating cylinder, the motion of the fluid in the latter case being supposed to be in two dimensions. The sphere is chosen as the best representative of a bell, among the few geometrical forms of body for which the problem can be solved. The cylinder is chosen as the representative of a vibrating string. In the case of the sphere the problem is identical with that solved by Poisson in his memoir "*Sur les mouvements simultanés d'un pendule et de l'air environnant*"*; but the solution is discussed with a totally different object in view, and is obtained from the beginning, to avoid the needless complexity introduced by taking account of the initial circumstances, instead of supposing the motion already going on.

A. Solution of the Problem in the case of a Vibrating Sphere.

Suppose an elastic solid, spherical externally in its undisturbed position, to vibrate in the manner of a bell, the amplitude of vibration being very small. Suppose it surrounded by a homo-

* *Mémoires de l'Académie des Sciences*, vol. xi. p. 521.

geneous gas, which is at rest except in so far as it is set in motion by the sphere; and let it be required to determine the motion of the gas in terms of that of the sphere supposed given. We may evidently for the purposes of the present problem suppose the gas not to be subject to the action of external forces.

Let the gas be referred to the rectangular axes of x, y, z , and let u, v, w be the components of the velocity. Since the gas is at rest except as to the disturbance communicated to it from the sphere, u, v, w are by a well-known theorem the partial differential coefficients with respect to x, y, z of a function ϕ of the coordinates; and if a^2 be the constant expressing the ratio of the small variations of pressure to the corresponding small variations of density, we must have

$$\frac{d^2\phi}{dt^2} = a^2 \left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} \right); \quad . \quad . \quad . \quad (1)$$

and if s be the small condensation,

$$s = -\frac{1}{a^2} \frac{d\phi}{dt}.$$

As we have to deal with a sphere, it will be convenient to refer the gas to polar coordinates r, θ, ω , the origin being in the centre. In terms of these coordinates, equation (1) becomes

$$\frac{d^2\phi}{dt^2} = a^2 \left\{ \frac{d^2\phi}{dr^2} + \frac{2}{r} \frac{d\phi}{dr} + \frac{1}{r^2 \sin \theta} \frac{d}{d\theta} \left(\sin \theta \frac{d\phi}{d\theta} \right) + \frac{1}{r^2 \sin^2 \theta} \frac{d^2\phi}{d\omega^2} \right\}; \quad (2)$$

and if u', v', w' be the components of the velocity along the radius vector and in two directions perpendicular to the radius vector, the first in and the second perpendicular to the plane in which θ is measured,

$$u' = \frac{d\phi}{dr}, \quad v' = \frac{1}{r} \frac{d\phi}{d\theta}, \quad w' = \frac{1}{r \sin \theta} \frac{d\phi}{d\omega}. \quad . \quad . \quad (3)$$

Let c be the radius of the sphere, and V the velocity of any point of its surface resolved in a direction normal to the surface, V being a given function of t, θ , and ω ; then we must have

$$\frac{d\phi}{dr} = V \text{ when } r = c. \quad . \quad . \quad . \quad (4)$$

Another condition, arising from what takes place at a great distance from the sphere, will be considered presently.

The sphere vibrating under the action of its elastic forces, its motion will be periodic, expressed so far as the time is concerned partly by the sine and partly by the cosine of an angle proportional to the time, suppose mat . Actually the vibrations will

slowly die away, in consequence partly of the imperfect elasticity of the sphere, partly of communication of motion to the gas; but for our present purpose this need not be taken into account. Moreover there will in general be a series of periodic disturbances coexisting, corresponding to different periodic times; but these may be considered separately. We might therefore assume

$$V = U \sin mat + U' \cos mat ;$$

but it will materially shorten the investigation to employ an imaginary exponential instead of circular functions. If we take

$$V = U e^{imat}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where i is an abbreviation for $\sqrt{-1}$, and determine ϕ by the conditions of the problem, the real and imaginary parts of ϕ and V must satisfy all those conditions separately; and therefore we may take the real parts alone, or the coefficients of i or $\sqrt{-1}$ in the imaginary parts, or any linear combination of these even after having changed the arbitrary constants which enter into the expression of the motion of the sphere, as the solution of the problem, according to the way in which we conceive the given quantity V expressed.

The function ϕ will be periodic in a similar manner to V , so that we may take

$$\phi = \psi e^{imat}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

As regards the periodicity merely, we might have had a term involving e^{-imut} as well as that written above; but it will be readily seen that in order to satisfy the equation of condition (4) the sign of the index of the exponential in ϕ must be the same as in V .

On substituting in (2) the expression for ϕ given by (6), the factor involving t will divide, and we shall get for the determination of ψ a partial differential equation free from t . Now ψ may be expanded in a series of Laplace's Functions so that

$$\psi = \psi_0 + \psi_1 + \psi_2 + \dots \quad (7)$$

Substituting in the differential equation just mentioned, taking account of the fundamental equation

$$\frac{1}{\sin \theta} \frac{d}{d\theta} \left(\sin \theta \frac{d\psi_n}{d\theta} \right) + \frac{1}{\sin^2 \theta} \frac{d^2 \psi_n}{d\omega^2} = -n(n+1) \psi_n,$$

and equating to zero the sum of the Laplace's Functions of the same order, we find

$$\frac{d^2\psi_n}{dr^2} + \frac{2}{r} \frac{d\psi_n}{dr} - \frac{n(n+1)}{r^2} \psi_n + m^2 \psi_n = 0.$$

This equation belongs to a known integrable form. The integral

is

$$\gamma\psi_n = u_n e^{-imr} \left\{ 1 + \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1)n(n+1)(n+2)}{2 \cdot 4(imr)^2} + \dots \right\} \\ + u'_n e^{imr} \left\{ 1 - \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1)n(n+1)(n+2)}{2 \cdot 4(imr)^2} - \dots \right\},$$

u_n and u'_n being evidently Laplace's Functions of the order n , since that is the case with ψ_n .

It will be convenient to take next the condition which has to be satisfied at a great distance from the sphere. When r is very large, the series within braces may be reduced to their first terms 1, and we shall have

$$r\phi = e^{im(at-r)} \Sigma u_n + e^{im(at+r)} \Sigma u'_n.$$

The first of these terms denotes a disturbance travelling outwards from the centre, the second a disturbance travelling towards the centre, the amplitude of vibration in both cases, for a given phase, varying inversely as the distance from the centre. In the problem before us there is no disturbance travelling towards the centre, and therefore $\Sigma u'_n = 0$, which requires that each function u'_n should separately be equal to zero. We have therefore simply

$$r\psi_n = u_n e^{-imr} \left\{ 1 + \frac{n(n+1)}{2 \cdot imr} + \frac{(n-1) \dots (n+2)}{2 \cdot 4(imr)^2} + \dots + \frac{1 \cdot 2 \cdot 3 \dots 2n}{2 \cdot 4 \cdot 6 \dots 2n(imr)^n} \right\}, \quad (8)$$

or, if we choose to reverse the series,

$$r\psi_n = u_n e^{-imr} \frac{1 \cdot 3 \cdot 5 \dots (2n-1)}{(imr)^n} \left\{ 1 + \frac{2n}{1 \cdot 2n} imr + \frac{(2n-2)2n}{1 \cdot 2(2n-1)2n} (imr)^2 \dots + \frac{2 \cdot 4 \cdot 6 \dots 2n}{1 \cdot 2 \cdot 3 \dots 2n} (imr)^n \right\} \quad (9).$$

Putting for shortness $f_n(r)$ for the multiplier of $u_n e^{-imr}$ in the right-hand member of (8) or (9), we shall have

$$\phi = \frac{1}{r} e^{im(at-r)} \Sigma u_n f_n(r).$$

It remains to satisfy the equation of condition (4). Put for shortness

$$\frac{d}{dr} \left\{ \frac{1}{r} e^{-imr} f_n(r) \right\} = - \frac{1}{r^2} e^{-imr} F_n(r),$$

so that

$$F_n(r) = (1 + imr) f_n(r) - r f_n'(r), \quad . \quad . \quad . \quad (10)$$

and suppose U expanded in a series of Laplace's Functions,

$$U_0 + U_1 + U_2 + \dots;$$

then substituting and equating the functions of the same order on the two sides of the equation, we have

$$U_n = -\frac{1}{c^2} e^{-ime} F_n(c) u_n,$$

and therefore

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \sum \frac{U_n}{F_n(c)} f_n(r). \quad . \quad . \quad . \quad (11)$$

This expression contains the solution of the problem, and it remains only to discuss it.

At a great distance from the sphere the function $f_n(r)$ becomes ultimately equal to 1, and we have

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \sum \frac{U_n}{F_n(c)}. \quad . \quad . \quad . \quad . \quad (12)$$

It appears from (3) that the component of the velocity along the radius vector is of the order r^{-1} , and that in any direction perpendicular to the radius vector of the order r^{-2} , so that the lateral motion may be disregarded except in the neighbourhood of the sphere.

In order to examine the influence of the lateral motion in the neighbourhood of the sphere, let us compare the actual disturbance at a great distance with what it would have been if all lateral motion had been prevented, suppose by infinitely thin conical partitions dividing the fluid into elementary canals, each bounded by a conical surface having its vertex at the centre.

On this supposition the motion in any canal would evidently be the same as it would be in all directions if the sphere vibrated by contraction and expansion of the surface the same all round and such that the normal velocity of the surface was the same as it is at the particular point in which the canal in question abuts on the surface. Now if U were constant the expansion of U would be reduced to its first term U_0 , and seeing that $f_0(r) = 1$ we should have from (11)

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \frac{U_0}{F_0(c)}.$$

This expression will apply to any particular canal if we take U_0 to denote the normal velocity at the sphere's surface for that particular canal; and therefore to obtain an expression applicable at once to all the canals we have merely to write U for U_0 . To facilitate a comparison with (11) and (12) I shall, however, write

ΣU_n for U . We have then

$$\phi = -\frac{c^2}{r} e^{im(at-r+c)} \frac{\Sigma U_n}{F_0(c)}. \quad (13)$$

It must be remembered that this is merely an expression applicable at once to all the canals, the motion in each of which takes place wholly along the radius vector; and accordingly the expression is not to be differentiated with respect to θ or ω with the view of applying the formula (3).

On comparing (13) with the expression for the function ϕ in the actual motion at a great distance from the sphere (12), we see that the two are identical, with the exception that U_n is divided by two different constants, namely $F_0(c)$ in the former case and $F_n(c)$ in the latter. The same will be true of the leading terms (or those of the order r^{-1}) in the expressions for the condensation and velocity*. Hence if the mode of vibration of the sphere is such that the normal velocity of its surface is expressed by a Laplace's Function of any one order, the disturbance at a great distance from the sphere will vary from one direction to another according to the same law as if lateral motion had been prevented, the amplitude of excursion at a given distance from the centre varying in both cases as the amplitude of excursion, in a normal direction, of the surface of the sphere itself. The only difference is that expressed by the symbolic ratio $F_n(c):F_0(c)$. If we suppose $F_n(c)$ reduced to the form $\mu_n (\cos \alpha_n + \sqrt{-1} \sin \alpha_n)$, the amplitude of vibration in the actual case will be to that in the supposed case as μ_0 to μ_n , and the phases in the two cases will differ by $\alpha_0 - \alpha_n$.

If the normal velocity of the surface of the sphere be not expressible by a single Laplace's Function, but only by a series, finite or infinite, of such functions, the disturbance at a given great distance from the centre will no longer vary from one direction to another according to the same law as the normal velocity of the surface of the sphere, since the modulus μ_n and likewise the amplitude α_n of the imaginary quantity $F_n(c)$ vary with the order of the function.

Let us now suppose the disturbance expressed by a Laplace's Function of some one order, and seek the numerical value of the

* Of course it would be true if the *complete* differential coefficients with respect to r of the right-hand members of (12) and (13) were taken; but then the former does not give the velocity u' except as to its leading term, since (12) has been deduced from the exact expression (11) by reducing $f_n(r)$ to its first term 1; nor again is it true, except as to terms of the order r^{-1} , of the actual motion of the unimpeded fluid that the whole velocity is in the direction of the radius vector.

alteration of intensity at a distance, produced by the lateral motion which actually exists.

The intensity will be measured by the *vis viva* produced in a given time, and consequently will vary as the density multiplied by the velocity of propagation multiplied by the square of the amplitude of vibration. It is the last factor alone that is different from what it would have been if there had been no lateral motion. The amplitude is altered in the proportion of μ_0 to μ_n ; so that if

$$\frac{\mu_n^2}{\mu_0^2} = I_n,$$

I_n is the quantity by which the intensity which would have existed if the fluid had been hindered from lateral motion has to be divided.

For the first five orders the values of the function $F_n(c)$ are as follows:—

$$F_0(c) = imc + 1,$$

$$F_1(c) = imc + 2 + imc,$$

$$F_2(c) = imc + 4 + \frac{9}{imc} + \frac{9}{(imc)^2},$$

$$F_3(c) = imc + 7 + \frac{27}{imc} + \frac{60}{(imc)^2} + \frac{60}{(imc)^3},$$

$$F_4(c) = imc + 11 + \frac{65}{imc} + \frac{240}{(imc)^2} + \frac{525}{(imc)^3} + \frac{525}{(imc)^4}.$$

If λ be the length of the sound-wave corresponding to the period of the vibration, $m = \frac{2\pi}{\lambda}$; so that mc is the ratio of the circumference of the sphere to the length of a wave. If we suppose the gas to be air and λ to be 2 feet, which would correspond to about 550 vibrations in a second, and the circumference $2\pi c$ to be 1 foot (a size and pitch which would correspond with the case of a common house bell), we shall have $mc = \frac{1}{2}$. The following Table gives the values of the square of the modulus and of the ratio I_n for the functions $F_n(c)$ of the first five orders, for each of the values $4, 2, 1, \frac{1}{2}$, and $\frac{1}{4}$ of mc . It will presently appear why the Table has been extended further in the direction of values greater than $\frac{1}{2}$ than it has in the opposite direction. Five significant figures at least are retained.

$mc.$	$n=0.$	$n=1.$	$n=2.$	$n=3.$	$n=4.$	
4	17	16.25	14.879	13.848	20.177	Values of μ_n^2
2	5	5	9.3125	80	1495.8	
1	2	5	89	3965	300137	
0.5	1.25	16.25	1330.2	236191	72086371	
0.25	1.0625	64.062	20878	14837899	18160×10^6	
4	1	0.95588	0.87523	0.81459	1.1869	Values of I_n
2	1	1	1.8625	16	299.16	
1	1	2.5	44.5	1982.5	150068	
0.5	1	13	1064.2	188953	57669097	
0.25	1	60.294	19650	13965×10^3	17092×10^6	

When $mc = \infty$ we get from the analytical expressions $I_n = 1$. We see from the Table that when mc is somewhat large I_n is liable to be a little less than 1, and consequently the sound to be a little more intense than if lateral motion had been prevented. The possibility of this is explained by considering that the waves of condensation spreading from those compartments of the sphere which at a given moment are vibrating positively, *i. e.* outwards, after the lapse of a half period may have spread over the neighbouring compartments, which are now in their turn vibrating positively, so that these latter compartments in their outward motion work against a somewhat greater pressure than if each compartment had opposite to it only the vibration of the gas which it had itself occasioned; and the same explanation applies *mutatis mutandis* to the waves of rarefaction. However, the increase of sound thus occasioned by the existence of lateral motion is but small in any case, whereas when mc is somewhat small I_n increases enormously, and the sound becomes a mere nothing compared with what it would have been had lateral motion been prevented.

The higher the order of the function, the greater will be the number of compartments, alternately positive and negative as to their mode of vibration at a given moment, into which the surface of the sphere will be divided. We see from the Table that for a given periodic time as well as radius the value of I_n becomes considerable when n is somewhat high. However, practically vibrations of this kind are produced when the elastic sphere executes, not its principal, but one of its subordinate vibrations, the pitch corresponding to which rises with the order of the vibration, so that m increases with that order. It was for this reason that the Table was extended from $mc = 0.5$ further in the direction of high pitch than low pitch, namely, to three octaves higher and only one octave lower.

When the sphere vibrates symmetrically about the centre, *i. e.* so that any two opposite points of the surface are at a given

moment moving with equal velocities in opposite directions, or (more generally) when the mode of vibration is such that there is no change of position of the centre of gravity of the volume, there is no term of the order 1. For a sphere vibrating in the manner of a bell the principal vibration is that expressed by a term of the order 2, to which I shall now more particularly attend.

Putting, for shortness, $m^2c^2=q$, we have

$$\mu_0^2 = q + 1, \quad \mu_2^2 = (q^{\frac{1}{2}} - 9q^{-\frac{1}{2}})^2 + \left(4 - \frac{9}{q}\right)^2 = q - 2 + \frac{9}{q} + \frac{81}{q^2},$$

$$I_2 = \frac{q^3 - 2q^2 + 9q + 81}{q^2(q + 1)}.$$

The minimum value of I_2 is determined by

$$q^3 - 6q^2 - 84q - 54 = 0,$$

giving approximately

$$q = 12.859, \quad mc = 3.586, \quad \mu_0^2 = 13.859, \quad \mu_2^2 = 12.049,$$

$$I_2 = .86941;$$

so that the utmost increase of sound produced by lateral motion amounts to about 15 per cent.

I come now more particularly to Leslie's experiments. Nothing is stated as to the form, size, or pitch of his bell; and even if these had been accurately described, there would have been a good deal of guesswork in fixing on the size of the sphere which should be considered the best representative of the bell. Hence all we can do is to choose such values for m and c as are comparable with the probable conditions of the experiment.

I possess a bell, belonging to an old bell-in-air apparatus, which may probably be somewhat similar to that used by Leslie. It is nearly hemispherical, the diameter is 1.96 inch, and the pitch an octave above the middle C of a piano. Taking the number of vibrations 1056 per second, and the velocity of sound in air 1100 feet per second, we have $\lambda = 12.5$ inches. To represent the bell by a sphere of the same radius would be very greatly to underrate the influence of local circulation, since near the mouth the gas has but a little way to get round from the outside to the inside, or the reverse. To represent it by a sphere of half the radius would still apparently be to underrate the effect. Nevertheless for the sake of rather underestimating than exaggerating the influence of the cause here investigated, I will make these two suppositions successively, giving respectively $c = .98$ and $c = .49$, $mc = .4926$ and $mc = .2463$ for air.

If it were not for lateral motion the intensity would vary from gas to gas in the proportion of the density into the velocity

of propagation, and therefore as the pressure into the square root of the density under a standard pressure, if we take the factor depending on the development of heat as sensibly the same for the gases and gaseous mixtures with which we have to deal. In the following Table the first column gives the gas, the second the pressure p , in atmospheres, the third the density D under the pressure p , referred to the density of air at the atmospheric pressure as unity, the fourth, Q_r , what would have been the intensity had the motion been wholly radial, referred to the intensity in air at atmospheric pressure as unity, or, in other words, a quantity varying as $p \times$ (the density at pressure 1) ^{$\frac{1}{2}$} . Then follow the values of q , I_2 , and Q , the last being the actual intensity referred to air as before.

Gas.	p .	D .	Q_r .	$c=.98$.			$c=.49$.		
				q .	I_2 .	Q .	q .	I_2 .	Q .
Air	1	1	1	.2427	1136	1	.06067	20890	1
Hydrogen	1	.0690	.2627	.01674	284700	.001048	.004186	4604000	.001191
Air, rarefied01	.01	.01	.2427	1136	.01	.06067	20890	.01
Air filled with H... 1		.0783	.2798	.0190	220600	.001440	.004751	3572000	.001637
Air of same density.	.0783	.0783	.0783	.2427	1136	.0783	.06067	20890	.0783
Air rarefied $\frac{1}{2}$5	.5	.5	.2427	1136	.5	.06067	20890	.5
Air filled with H... 1		.5345	.7311	.1297	4322	.1921	.0324	74890	.2039

An inspection of the numbers contained in the columns headed Q will show that the cause here investigated is amply sufficient to account for the facts mentioned by Leslie.

It may be noticed that while q is 4 times as small, and I_2 is 16 or 18 times as large, for $c=.49$ as for $c=.98$, there is no great difference in the values of Q in the two cases for hydrogen and mixtures of hydrogen and air in given proportions. This arises from the circumstance that q is sufficiently small to make the last terms in μ_0^2 and μ_2^2 , namely, 1 and $81q^{-2}$, the most important, so that I_n does not greatly differ from $81q^{-2}$. If this result had been exact instead of approximate, the intensity in different gases, supposed for simplicity to be at a common pressure, would have varied as $D^{\frac{5}{2}}$; and it will be found that for the cases in which $p=1$ the values of Q in the above Table, especially those in the last column, do not greatly deviate from this proportion. But the simplicity of this result depends on two things. First, the vibration must be expressed by a Laplace's Function of the order 2; for a different order the power of D would have been different; and this is just one of the points respecting which we cannot infer what would be true of a bell of the ordinary shape from what we have proved for a sphere.

Secondly, the radius must be sufficiently small, or the pitch sufficiently low, to make q small; at the other extremity of the scale, in which c is supposed to be very large, or λ very small, Q varies nearly as $D^{\frac{1}{2}}$ instead of $D^{\frac{5}{2}}$, whatever be the order of the Laplace's Function. Hence no simple relation can be expected between the numbers furnished by experiment and the numerical constants of the gas in such experiments as those of M. Perolle*, in which the same bell was rung in succession in different gases.

B. Solution of the Problem in the case of a Vibrating Cylinder.

I will here suppose the motion to be in two dimensions only. In the case of a vibrating string, which I have mainly in view, it is true that the amplitude of excursion of the string varies sensibly on proceeding even a moderate distance along it, and that the propagation of the sound-wave produced by no means takes place in two dimensions only. But the question how far a sound-wave is produced at all, and how far the displacement of the gas by the cylinder merely produces a local motion to and fro, is decided by what takes place in the immediate neighbourhood of the string, such as within a distance of a few diameters; and though the sound-wave, when once produced, in its subsequent progress diverges in three dimensions, the same takes place with the hypothetical sound-wave which would be produced if lateral motion were prevented; so that the comparison which it is the object of the investigation to institute is not affected thereby.

Assuming, then, the motion to be in two dimensions, and referring the fluid to polar coordinates, r, θ , r being measured from the axis of the undisturbed cylinder, we shall have for the fundamental equation derived from (1),

$$\frac{d^2\phi}{dt^2} = a^2 \left\{ \frac{d^2\phi}{dr^2} + \frac{1}{r} \frac{d\phi}{dr} + \frac{1}{r^2} \frac{d^2\phi}{d\theta^2} \right\}; \quad . \quad . \quad . \quad (14)$$

and if u', v' be the components of the velocity along and perpendicular to the radius vector,

$$u' = \frac{d\phi}{dr} \quad v' = \frac{1}{r} \frac{d\phi}{d\theta}.$$

If c be the radius of the cylinder, and V the normal component of the velocity of the surface of the cylinder, we must have

$$\frac{d\phi}{dr} = V \text{ when } r = c.$$

As before, I will suppose the motion of the cylinder, and consequently of the fluid, to be regularly periodic; but instead of

* *Mémoires de l'Académie des Sciences de Turin*, vol. iii. (1786-7), *Mém. des Correspondans*, p. 1.

using circular functions directly, I will employ the imaginary exponential e^{imat} , i denoting as before $\sqrt{-1}$, and will put accordingly $V = e^{imat}U$, U being a given function of θ , and $\phi = \psi e^{imat}$. For a given value of r , ψ may by a known theorem be developed in a series of sines and cosines of θ and its multiples, and therefore for general values of r can be so developed, the coefficients being functions of r . If ψ'_n be the coefficient of $\cos n\theta$ or $\sin n\theta$, we find

$$\frac{d^2\psi_n}{dr^2} + \frac{1}{r} \frac{d\psi_n}{dr} - \frac{n^2}{r^2} \psi_n + m^2 \psi_n = 0. \quad (15)$$

If we suppose the normal velocity of the surface of the cylinder to vary in a given manner from one generating line to another, so that U is a given function of θ , we may expand U in a series of the form

$$U = U_0 + U_1 \cos \theta + U_2 \cos 2\theta + \dots \\ + U'_1 \sin \theta + U'_2 \sin 2\theta + \dots$$

On applying now the equation of condition which has to be satisfied at the surface of the cylinder, we see that a term $U_n \cos n\theta$ or $U'_n \sin n\theta$ of the n th order in the expression for U will introduce a function ψ_n of the same order in the general expression for ϕ . Now the only case of interest relating to an infinite cylinder is that of a vibrating string, in which the cylinder moves as a whole. The vibration may be regarded as compounded of the vibrations in any two rectangular planes passing through the axis, the phases of the component vibrations, it may be, being different. These component vibrations may be treated separately, and thus it will suffice to suppose the vibration confined to one plane, which we may take to be that from which θ is measured. We shall accordingly have

$$U = U_1 \cos \theta,$$

U_1 being a given constant; and the only function ψ_n which will appear in the general expression for ϕ will be that of the order 1. Besides this we shall have to investigate, for the sake of comparison, an ideal vibration in which the cylinder alternately contracts and expands in all directions alike, and for which accordingly U is a constant U_0 . Hence the equation (15) need only be considered for the values 0 and 1 of n .

For general values of n the equation (15) is easily integrated in the form of infinite series according to ascending powers of r . The result is

$$\psi_n = A r^n \left\{ 1 - \frac{m^2 r^2}{2(2+2n)} + \frac{m^4 r^4}{2 \cdot 4(2+2n)(4+2n)} - \dots \right\} \\ + B r^{-n} \left\{ 1 - \frac{m^2 r^2}{2(2-2n)} + \frac{m^4 r^4}{2 \cdot 4(2-2n)(4-2n)} - \dots \right\} \quad (16)$$

When n is any integer the integral as it stands becomes illusory ; but the complete integral, which in this case assumes a special form, is readily obtained as a limiting case of the complete integral for general values of n .

The series in (16) are convergent for any value of r however great, but they give us no information of what becomes of the functions for very large values of r .

When r is very large, the equation (15) becomes approximately

$$\frac{d^2\psi_n}{dr^2} + m^2\psi_n = 0,$$

the integral of which is $\psi_n = Re^{-imr} + R'e^{imr}$, where R and R' are constant. This suggests putting the complete integral of (15) under the same form, R and R' being now functions of r , which, when r is large, vary but slowly, that is, remain nearly constant when r is altered by only a small multiple of λ . Assuming for R and R' series of the form $Ar^\alpha + Br^\beta + Cr^\gamma \dots$, where $\alpha, \beta, \gamma \dots$ are in decreasing order algebraically, and determining the indices and coefficients so as to satisfy (15), we get for another form of the complete integral

$$\left. \begin{aligned} \psi_n = & C(imr)^{-\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2 - 4n^2}{1 \cdot 8imr} + \frac{(1^2 - 4n^2)(3^2 - 4n^2)}{1 \cdot 2(8imr)^2} \right. \\ & \left. - \frac{(1^2 - 4n^2)(3^2 - 4n^2)(5^2 - 4n^2)}{1 \cdot 2 \cdot 3(8imr)^3} + \dots \right\} \\ & + D(imr)^{-\frac{1}{2}} e^{imr} \left\{ 1 + \frac{(1^2 - 4n^2)}{1 \cdot 8imr} + \frac{(1^2 - 4n^2)(3^2 - 4n^2)}{1 \cdot 2(8imr)^2} \right. \\ & \left. + \frac{(1^2 - 4n^2)(3^2 - 4n^2)(5^2 - 4n^2)}{1 \cdot 2 \cdot 3(8imr)^3} + \dots \right\} \end{aligned} \right\} \quad (17)$$

These series, though ultimately divergent, begin by converging rapidly when r is large, and may be employed with great advantage when r is large, if we confine ourselves to the converging part. Moreover we have at once $D=0$ as the condition to be satisfied at a great distance from the cylinder. If mc were large we might employ the second form of integral in satisfying the condition at the surface of the cylinder, and the problem would present no further difficulty. But practically in the case of vibrating strings mc is a very small fraction, the series (16) are rapidly convergent, and the series (17) cannot be employed. To complete the solution of the problem, therefore, it is essential to express the constants A and B in terms of C and D , or at any rate to find the relation between A and B imposed by the condition $D=0$.

This may be effected by means of the complete integral of (15)

expressed in the form of a definite integral. For $n=0$ we know that

$$\psi_0 = \int_0^{\frac{\pi}{2}} \{E + F \log(r \sin^2 \zeta)\} \cos(mr \cos \zeta) d\zeta \quad (18)$$

is a third form of the integral of (15). It is not difficult to deduce from this the integral of (15) in a similar form for any integral value of n . Assuming

$$\psi_n = r^\alpha \int r^\beta \chi_n dr,$$

and substituting in (15), we have

$$r^{\alpha+\beta} \frac{d\chi_n}{dr} + (2\alpha + \beta + 1)r^{\alpha+\beta-1} \chi_n + (\alpha^2 - n^2)r^{\alpha-2} \int r^\beta \chi_n dr + m^2 r^\alpha \int r^\beta \chi_n dr = 0.$$

Assume

$$\alpha^2 - n^2 = 0, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

divide the equation by r^α , differentiate with respect to r , and then divide by r^β . The result is

$$\frac{d^2 n \chi_n}{dr^2} + \frac{2\alpha + 2\beta + 1}{r} \frac{d\chi_n}{dr} + (2\alpha + \beta + 1)(\beta - 1) \frac{\chi_n}{r^2} + m^2 \chi_n = 0.$$

This equation will be of the same form as (15), provided

$$\alpha + \beta = 0,$$

which reduces the coefficient of the last term but one to $-(\alpha + 1)^2$. In order that this coefficient may be increased we must choose the positive root of (19), namely n , which I will suppose positive. Hence

$$\psi_n = r^n \int r^{-n} \chi_n dr \quad . \quad . \quad . \quad . \quad . \quad (20)$$

gives

$$\frac{d^2 \chi_n}{dr^2} + \frac{1}{r} \frac{d\chi_n}{dr} - \frac{(n+1)^2}{r^2} \chi_n + m^2 \chi_n = 0,$$

the same equation as that for the determination of ψ_{n+1} . Hence, expressing χ_n in terms of ψ_n from (20), writing $n-1$ for n , and replacing χ_{n-1} by ψ_n , we have

$$\psi_n = r^{n-1} \frac{d}{dr} r^{-(n-1)} \psi_{n-1},$$

a formula of reduction which, when n is integral, serves to express ψ_n in terms of ψ_0 . We have

$$\psi_n = r^n \left(\frac{1}{r} \frac{d}{dr} \right)^n \psi_0 \quad . \quad . \quad . \quad . \quad . \quad (21)$$

an equation which when applied to (18) gives the complete inte-

gral of (15) for integral values of n in the form of a definite integral.

Let us attend now more particularly to the case of $n=0$. The equation (16) is of the form $\psi_n = Af(n) + Bf(-n)$, $f(n)$ containing r as well as n . Expanding by Maclaurin's theorem, we have

$$\psi_n = (A+B)f(0) + (A-B)f'(0)n + (A+B)f''(0)\frac{n^2}{1.2} + \dots$$

Writing A for $A+B$, $n^{-1}B$ for $A-B$, and then making n vanish, we have

$$\psi_0 = Af(0) + Bf'(0),$$

or

$$\psi_0 = (A+B \log r) \left\{ 1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 4^2} - \frac{m^6 r^6}{2^2 4^2 6^2} + \dots \right\} + B \left\{ \frac{m^2 r^2}{2^2} S_1 - \frac{m^4 r^4}{2^2 4^2} S_2 + \frac{m^6 r^6}{2^2 4^2 6^2} S_3 - \dots \right\}, \quad (22)$$

where

$$S_n = 1^{-1} + 2^{-1} + 3^{-1} \dots + n^{-1}.$$

The integral in the form (17) assumes no peculiar shape when n is integral, and we have at once

$$\psi_0 = C(imr)^{-\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2}{1.8mr} + \frac{1^2 3^2}{1.2(8mr)^2} - \frac{1^2 3^2 5^2}{1.2.3(8imr)^3} + \dots \right\} + D(imr)^{-\frac{1}{2}} e^{imr} \left\{ 1 + \frac{1^2}{1.8imr} + \frac{1^2 3^2}{1.2(8imr)^2} - \frac{1^2 3^2 5^2}{1.2.3(8imr)^3} + \dots \right\} \quad (23)$$

I have explained at length the mode of dealing with such functions, and especially of connecting the arbitrary constants in the ascending and descending series, in two papers published in the Transactions of the Cambridge Philosophical Society*, in the second of which the connexion of the constants is worked out in this very example. To these I will refer, merely observing that while it is perfectly easy to connect A , B with E , F , the connexion of C , D with E , F involves some extremely curious points of analysis. The result of eliminating E , F be-

* "On the Numerical Calculation of a Class of Definite Integrals and Infinite Series," vol. ix. p. 166, and "On the Discontinuity of Arbitrary Constants which appear in Divergent Developments," vol. x. p. 105. A supplement to the latter paper has recently been read before the Cambridge Philosophical Society.

tween the two equations connecting A, B with E, F and the two connecting C, D with E, F is given, except as to notation, in the two equations (41) of my second paper. To render the notation identical with that of the former paper, it will be sufficient to write $A - B \log(im) + B \log(imr)$ for $A + B \log r$, and x for imr . The equations referred to may be simplified by the introduction of Euler's constant γ , the value of which is $\cdot 57721566$ &c., since it is known that

$$\pi^{-\frac{1}{2}} \Gamma'(\frac{1}{2}) + \log 4 + \gamma = 0,$$

$\Gamma'(x)$ denoting the derivative of the function $\Gamma(n)$. Putting

$$A - B \log im = A',$$

we have by equations (41) of the second paper referred to

$$C = (2\pi)^{-\frac{1}{2}} \{iA' + [(\log 2 - \gamma)i - \pi]B\}, \quad . \quad . \quad . \quad (24)$$

$$D = (2\pi)^{-\frac{1}{2}} \{A' + (\log 2 - \gamma)B\}, \quad . \quad . \quad . \quad . \quad (25)$$

i being written for $\sqrt{-1}$. It is shown in that paper that these values of C, D hold good when the amplitude of the imaginary variable x lies between the limits 0 and π , or that of r (supposed to be imaginary) between the limits $-\frac{\pi}{2}$ and $\frac{\pi}{2}$, but in crossing either of these limits one or other of the constants C, D is changed. In the investigation of the present paper r is of course real.

We have now

$$A' = A - B \log im = (\gamma - \log 2)B$$

for the relation between A and B arising from the condition that the motion is propagated outwards from the cylinder; and substituting in (22), we have for the value of N_0 subject to this condition

$$\psi_0 = B \left(\gamma + \log \frac{imr}{2} \right) \left(1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 4^2} - \dots \right) \left\{ \begin{array}{l} + B \left(\frac{m^2 r^2}{2^2} S_1 - \frac{m^4 r^4}{2^2 4^2} S_2 + \dots \right); \end{array} \right\} \quad (26)$$

or expressed by means of the descending series,

$$\psi_0 = -B \left(\frac{\pi}{2imr} \right)^{\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{1^2}{1 \cdot 8imr} + \frac{1^2 3^2}{1 \cdot 2 (8imr)^2} - \frac{1^2 3^2 5^2}{1 \cdot 2 \cdot 3 (8imr)^3} + \dots \right\} \quad (27)$$

We have from (21)

$$\psi_1 = \frac{d\psi_0}{dr},$$

from which the complete integral of (15) for $n=1$ may be got from that for $n=0$. In the form (17) of the integral the parts arising from differentiation of the parts containing e^{-imr} and e^{imr} respectively will contain those same exponentials; and therefore the complete integral of (15) for $n=1$, subject to the condition that the part containing e^{imr} shall disappear, will be got by differentiating the complete integral for $n=0$ subject to that same condition. The form of the integral in the shape of a descending series is given at once by (17). Hence we get by differentiating (26) and (27), and at the same time changing the arbitrary constant by writing $B_1 m^{-1}$ for B,

$$\psi_1 = \frac{B}{mr} \left\{ 1 - \frac{m^2 r^2}{2^2} + \frac{m^4 r^4}{2^2 \cdot 4^2} - \dots \right. \\ \left. - B_1 \left(\gamma + \log \frac{imr}{2} \right) \left(\frac{mr}{2} - \frac{m^3 r^3}{2^2 \cdot 4} + \frac{m^5 r^5}{2^2 \cdot 4^2 \cdot 6} - \dots \right) \right. \\ \left. + B_1 \left(\frac{mr}{2} S_1 - \frac{m^3 r^3}{2^2 \cdot 4} S_2 + \frac{m^5 r^5}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots \right) \right\} \quad (28)$$

$$\psi_1 = B_1 \left(\frac{\pi i}{2mr} \right)^{\frac{1}{2}} e^{-imr} \left\{ 1 - \frac{-1 \cdot 3}{1 \cdot 8mr} + \frac{-1 \cdot 1 \cdot 3 \cdot 5}{1 \cdot 2 (8mr)^2} \right. \\ \left. - \frac{-1 \cdot 1 \cdot 3^2 \cdot 5 \cdot 7}{1 \cdot 2 \cdot 3 (8mr)^3} + \dots \right\} \quad (29)$$

To determine the arbitrary constants B_1 and B, the first belonging to the actual motion, the second to the motion which would take place if the fluid were confined by an infinite number of planes passing through the axis, we must have, as before, for $r=c$,

$$\frac{d\psi_0}{dr} = U_1, \quad \frac{d\psi_1}{dr} = U,$$

whence

$$\frac{cU_1}{B} = 1 - \frac{m^2 c^2}{2^2} + \frac{m^4 c^4}{2^2 \cdot 4^2} - \dots \\ - \left(\gamma + \log \frac{mc}{2} + i \frac{\pi}{2} \right) \left(\frac{m^2 c^2}{2} - \frac{m^4 c^4}{2^2 \cdot 4} + \dots \right) \\ + \frac{m^2 c^2}{2} S_1 - \frac{m^4 c^4}{2^2 \cdot 4} S_2 + \frac{m^6 c^6}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots \\ = F_0(mc) + i \frac{\pi}{2} f_0(mc), \text{ suppose; } \quad (30)$$

$$\left. \begin{aligned} \frac{cU_1}{B_1} = & -\frac{1}{mc} - \frac{3mc}{2^2} + \frac{7m^3c^3}{2^2 \cdot 4^2} - \frac{11m^5c^5}{2^2 \cdot 4^2 \cdot 6^2} + \dots \\ & - \left(\gamma + \log \frac{mc}{2} + i \frac{\pi}{2} \right) \left(\frac{mc}{2} - \frac{3m^3c^3}{2^2 \cdot 4} + \dots \right) \\ & + \frac{mc}{2} S_1 - \frac{3m^3c^3}{2^2 \cdot 4} S_2 + \frac{5m^5c^5}{2^2 \cdot 4^2 \cdot 6} S_3 - \dots \\ = & \frac{1}{mc} \left\{ F_1(mc) + i \frac{\pi}{2} f_1(mc) \right\}, \text{ suppose,} \end{aligned} \right\} \quad (31)$$

If I be the ratio of the intensities at a distance in the supposed and in the actual case, we see from (30) and (31) that I will be equal to the ratio of the squares of the moduli of B and B_1 , and we shall therefore have

$$I = \frac{\{4F_1(mc)\}^2 + \pi^2 \{f_1(mc)\}^2}{m^2c^2 [4\{F_0(mc)\}^2 + \pi^2 \{f_0(mc)\}^2]}. \quad (32)$$

For a piano-string corresponding to the middle C , c may be about $\cdot 02$ inch, and λ is about 25 inches. This gives $mc = \cdot 005027$. For such small values of mc I does not sensibly differ from $(mc)^{-2}$, which in the present case is 39571, so that the sound is nearly 40000 times as weak as it would have been if the motion of the particles of air had taken place in planes passing through the axis of the string. This shows the vital importance of sounding-boards in stringed instruments. Although the amplitude of vibration of the particles of the sounding-board is extremely small compared with that of the particles of the string, yet, as it presents a broad surface to the air, it is able to excite loud sonorous vibrations, whereas, were the string supported in an absolutely rigid manner, the vibrations which it could excite directly in the air would be so small as to be almost or altogether inaudible.

I may here mention a phenomenon which fell under my notice, and which is readily explained by the principles laid down in this paper. As I was walking one windy day on a road near Cambridge, on the other side of which ran a line of telegraph, my attention was attracted by a peculiar sound of extremely high pitch, which seemed to come from the opposite side of the road. On going over to ascertain the cause, I found that it came directly through the air from the telegraph-wires. On standing near a telegraph-post, the ordinary comparatively bass sound with which we are so familiar was heard, appearing to emanate from the post. On receding from the post the bass sound became feebler, and midway between two posts was quite inaudible. Nothing was then heard but the peculiar high-pitched sound, which appeared to emanate from the wires overhead. It had a peculiar metallic ring about it which the ear distinguished from the whistling of the wind in the twigs of a bush. Although the

telegraph ran for miles, it was only at one spot that the peculiar sound was noticed, and even there only in certain states of the wind. The wires seemed to be less curved than usual at the place in question, from which it may be inferred that they were there subject to an unusually great tension.

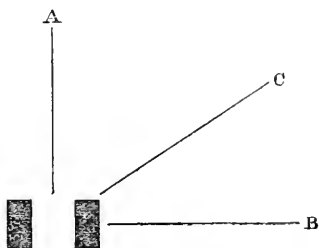
The explanation of the phenomenon is easy after what precedes. The wires were thrown into vibration by the wind, and a number of different vibrations, having different periodic times, coexisted. As regards the vibrations of comparatively long period, the air around the wires behaved nearly like an incompressible fluid, no sonorous vibrations of sensible amount were produced. These vibrations of the wires, however, were communicated to the posts, which being broad acted as sounding-boards, and thus sonorous vibrations of corresponding period were indirectly excited in the air. But as regards the vibrations of extremely short periodic time, the wires in spite of their narrowness were able, by acting directly on the air, to produce condensations and rarefactions of sensible amount.

The diameter of the telegraph-wire was about $\cdot 166$ inch; and if we take the C below the middle C of a piano for the representative of the pitch of the lower note, and a note five octaves higher for that of the higher, we have in the first case $\lambda = 50$ inches nearly, and in the second $\lambda = 50 \times 2^{-5}$, giving in the former case $mc = \cdot 01043$, and in the latter $mc = \cdot 3338$. The former of these values is so small that we may take $I = (mc)^{-2}$; in the latter case the formula (32) gives for I a value a little less than $(mc)^{-2}$. We find in the two cases $I = 9192$ and $I = 7\cdot 202$ respectively; so that in the former case the sound is more than 9000 times as feeble as that corresponding to the amplitude of vibration of the wire on the supposition of the absence of lateral motion, whereas in the latter case the actual intensity is nearly one-seventh of the full intensity corresponding to the amplitude.

The increase of sound produced by the stoppage of lateral motion may be prettily exhibited by a very simple experiment.

Take a tuning-fork, and holding it in the fingers after it has been made to vibrate, place a sheet of paper or the blade of a broad knife with its edge parallel to the axis of the fork, and as near to the fork as conveniently may be without touching. If the

plane of the obstacle coincide with either of the planes of symmetry of the fork, as represented in section at A or B, no effect is produced; but if it be placed in an intermediate position, such as C, the sound becomes much stronger.



LII. *Notes on the Chemical Geology of the Gold-fields of California.* By J. ARTHUR PHILLIPS.

[Concluded from p. 336.]

Hot Springs.

HOT and boiling springs are exceedingly numerous throughout California; and considerable accumulations of sulphur, together with evidences of extensive solfatara action, are met with in different sections of the State. To attempt even a mere enumeration of all the various localities where these phenomena have been observed would occupy more space than it is my intention to devote to the subject; and I shall therefore confine myself to a description of two or three of the more remarkable examples, selecting such only as appear to have the most direct bearing on some of the chemical and geological changes which have been produced, and are, to a certain extent, still going on in this portion of the American continent.

Borax Lake.—Lying about a mile beyond the ridge which borders Borax Lake on the north-east is a locality in which solfatara action is still exceedingly active, and where a large amount of sulphur has accumulated. This is called the "Sulphur Bank," and is of some six or seven acres in extent. It consists of a much decomposed volcanic rock traversed by innumerable fissures, through which steam and gas are constantly issuing, and over and through which large quantities of sulphur have been deposited in such a way that, at a little distance, the whole mass appears to consist of this substance. Into some of the cavities a pole may be inserted for several feet; and they are often lined with stalactites and beautiful crystallizations of sulphur.

This sulphur is being constantly deposited, and its deposition is attended by the evolution of aqueous vapour, carbonic and boracic acids; but this apparently takes place without the emission of sulphuretted hydrogen. The gaseous matters issuing from the crevices have usually a temperature of about 95° F., and appear to be the agency by which various mineral substances now deposited in the cavities were brought to the surface. Sulphur is deposited on the sides of the various fissures, either in groups of crystals, as stalactites, or in translucent amorphous masses of a beautiful yellow colour. It is sometimes intermixed with cinnabar, but more frequently with minute crystals of iron pyrites, and with pulverulent silica, blackened by the presence of a hydrocarbon having the appearance of coal-tar. With these, according to Dr. Oxland, are found silver and traces of gold*.

* Mining and Metallurgy of Gold and Silver, by J. Arthur Phillips, p. 11. E. and F. N. Spon: London.

On the sides of the cavities gelatinous silica is found coating chalcedony and opalescent silica in various stages of formation, from the gelatinous state to that of the hardest opal. This indurated silica is sometimes colourless, but is more frequently permeated by cinnabar and iron pyrites, or blackened by the tarry matter before referred to. Cinnabar is also found in striæ, and occasionally even in veins and concretionary masses of considerable thickness; where the bituminous matter occurs in the largest quantity, and the mass consequently becomes black and friable, cinnabar is replaced by globules of metallic mercury.

On the edge of Clear Lake near the sulphur bank is a hot spring, of which the outlet, even when the water is low, is partially beneath the lake, so that the amount which flows from it cannot be ascertained. Hot water, however, rises through the sand at various points, extending over a considerable area; and Mr. Moore, of San Francisco, who analyzed the water from this locality, has found that, in addition to chloride of sodium and carbonate of soda, it contains a large quantity of borax, and above a hundred grains of ammoniacal salts per gallon. Lying immediately at the foot of the sulphur bank are several springs of cold water, some of which contain carbonate and sulphate of soda, whilst others yield boracic acid. All these springs evolve large quantities of carbonic acid.

The chalcedony and semi-opal from this place, when examined under the microscope, are seen to enclose cubical pyrites, together with crystalline cinnabar; but the latter mineral appears to have been generally deposited in an amorphous state. Specimens of chalcedony taken from the fissures in the sulphur bank were, when first broken, so extremely soft as to readily receive an impression of the nail; but on reaching England they had become hardened, and had assumed the ordinary characteristics of that mineral.

Thin sections of these specimens showed a structure like that of fortification-agate, and were traversed by numerous fissures parallel to lines of incipient crystallization, besides which they were in places stained by oxide of iron.

Boiling Springs.—About fifteen miles east of Little Lake, situated in lat. $35^{\circ} 50'$ north and long. $18^{\circ} 10'$ west from Greenwich, 200 miles north of Los Angeles, are numerous boiling springs. The first group covers several acres of the southern declivity of a granitic hill, and is no longer in a state of great activity, although numerous apertures are giving off an abundance of steam and acid vapours, and small quantities of sulphur are being deposited around the various openings. In the immediate neighbourhood of these springs the granite has been reduced to the state of a soft hot mud very dangerous to walk over; and

a large area having become reduced to this pasty condition, it has flowed down the side of the elevation and become deposited in layers on the low ground near its base. It was also observed in one of the ravines that mud resulting from the upper springs had sometimes flowed over and choked those lower in the series; and this appears to have given rise to the formation of fresh openings, and the progressive movement of the area of greatest activity up the side of the declivity.

About a mile to the west of these springs a high granitic mountain is traversed by an immense dyke of obsidian, which, having broken through it, has flowed for a considerable distance into the valley.

At the eastern foot of the hill on which is situated the first group of springs are numerous others in a much more active state. Here the ground is covered over a large extent by innumerable cones of plastic mud, varying from a few inches to three feet in height; these rise above the surface of a barren steaming swamp, and give issue to jets of watery vapour and streams of boiling water. In some cases these springs, instead of issuing from small cones as above described, evolve large quantities of steam and gas under the surface of water contained in basin-shaped reservoirs produced by their agency in the decomposed granite in which they are situated. By these means are formed a multitude of seething caldrons, in which the rapid ebullition of water keeps the mud in a constant state of suspension; and this is either white or of a reddish colour, in accordance with the amount of ferruginous matters present. To the east, at a distance of some three or four miles, were seen high ranges of evidently basaltic hills, the whole district being totally uninhabited except by a few Indians, some of whom were met with collecting oxide of iron to be employed as paint.

The appliances at command in such localities are usually of a limited character; but I was enabled to ascertain by means of litmus-paper that the water of all the springs examined had an acid reaction; and having filled a pint bottle with water from one of the largest of the basins, it was brought to England for analysis.

This amount, although manifestly too small to allow of a very accurate analysis, was sufficient to admit of the estimation of all the principal constituents. The following are the respective quantities of the different bodies present, calculated on an imperial gallon of the filtered water:—

Contents per Imperial Gallon.

Silica	grs. 7·00
Alumina	7·70
Oxide of iron	traces
Lime	1·96
Magnesia	1·41
Potash	0·85
Soda	1·26
Chlorine	2·25
Sulphuric acid	25·86

The foregoing results may be rendered thus:—

Contents per Imperial Gallon.

Silica	7·00
Tersulphate of alumina	25·67
Sulphate of iron	trace
„ lime	4·76
„ potash	1·57
„ soda	2·87
„ magnesia	1·77
Chloride of magnesium	3·01
Free sulphuric acid	1·57
	<hr/> 48·22

It is somewhat remarkable that this water, issuing from the ground in a boiling state, should at the same time contain chlorides and free sulphuric acid; but it must be remembered that the acid is in an exceedingly dilute state, and that the water was collected immediately at the mouth of the aperture from which it emerged. It is probable that potash and soda may exist in the form of the bisulphates of those bases, since an excess of sulphuric acid is present.

It was observed that where these acid waters flowed away at a temperature of above 200° F. several low forms of vegetation (probably Confervaceæ) appeared to flourish, but to become less abundant and less vigorous where the temperature had become considerably reduced.

Steamboat Springs.—The most remarkable instance on the Pacific coast of the actual growth, on a large scale and at the present time, of mineral veins is probably that afforded by the boiling springs in Steamboat Valley, seven miles north-west of the great Comstock silver vein and Virginia city.

These springs are situated at a height of about 5000 feet above

the level of the sea, at the foot of the eastern declivity of the Sierra Nevada. The rock in this locality presents several parallel fissures, either giving out heated water or simply ejecting steam. The first principal group of crevices comprises five longitudinal openings extending in a straight line for a distance of 3500 feet. Their general direction is north 6° west, and they are comprised within a band of about 600 feet in width. The fissures are separated from one another by intervals of from 40 to 60 feet, have each a thickness of about 12 inches, and are severally connected with each other by lesser openings, which intersect the first nearly at right angles.

These crevices are often full of boiling water, which sometimes overflows and escapes in the form of a rivulet, whilst at others it does not flow over, but violent ebullition is heard to be taking place at a short distance below the surface. The fissures are partially filled by a siliceous incrustation which is being constantly deposited on the sides, whilst a longitudinal central crevice allows of the escape of boiling water or steam. On the most eastern of these lines of fracture are several active centres of eruption, from which boiling water is often ejected to a height of from 8 to 10 feet. These waters are alkaline, and contain, in addition to carbonate of soda, the sulphate of that base, with chloride of sodium. There is also everywhere an escape of carbonic acid, whilst from some places sulphuretted hydrogen is also evolved. These products give rise to the deposition of sulphur, silica, and oxide of iron. The silica forms semicrystalline bands parallel with the walls of the fissures; and spongy deposits accumulate around some of the points of most active emergence in such a way as to form small circular basins, of which the edges go on continually increasing in height, so that each finally presents the appearance of a small volcano of boiling water.

At a considerable distance to the west of these, a second group having the same origin is observed; but this is no longer traversed by hot water, although it still gives off steam and carbonic acid at various points. This large dyke of quartz commences at its southern extremity by a number of fissures which subsequently converge into a single crevice, of which the two walls are of silica, and extend a distance of considerably over a mile. At its northern extremity a central fissure still remains open; but in other localities it is for the most part obstructed by siliceous concretions. Towards the south the siliceous deposits extended beyond the edges of the cleft, and have accumulated on the surface of the ground to a thickness of several feet, and for a distance of from 80 to 90 feet on each side of the fissure. These deposits almost entirely consist of more or less hydrated silica.

This silica presents itself in the form of a compact rock, much resembling in appearance the quartz of ordinary veins, with chalcedony, and occasional nodules of hyalite. This rock is, to a certain extent, metalliferous, and, in addition to oxide of iron, contains oxide of manganese, together with iron and copper pyrites. M. Laur states that he discovered metallic gold in this deposit, although I was myself unable to detect its presence.

These phenomena of the Steamboat Valley appear to indicate that metalliferous veins may be produced by mineral waters, in the fissures through which they circulate. On taking into consideration the principal circumstances connected with these veins, it will be observed that their eruptive activity is by no means equal throughout their longitudinal extent. This activity is concentrated around certain points, which appear to be the sources from which are principally discharged the waters holding various mineral substances in solution, and whence they subsequently become distributed throughout the fissures existing in the rock. It has been observed by M. Laur* that the slowness with which these incrustations are formed in the interior of the cavities, as compared with the great mass of mineral waters which escape over the edge of the various fissures, also shows that a large proportion of the metalliferous emanations may thus have been carried beyond them ; so that, should they contain a non-oxidizable metal, such as gold, the superficial deposits may occupy a considerable surface and contain a great amount of wealth, although the vein from which it originally issued may be small and comparatively unimportant.

The rock enclosing the veins of Steamboat Springs is a granite, which in their vicinity is much decomposed, being often reduced to a cavernous skeleton of silica containing only a few scales of mica. The granite in this locality is poor in quartz, and chiefly consists of oligoclase, but contains magnetic oxide of iron, titaniferous iron, &c.

This rock, forming the bottom of Steamboat Valley, is overlain on its eastern and western flanks by a compact basalt containing magnetic oxide of iron together with various zeolitic minerals.

Examination of the siliceous deposit at Steamboat Springs.—A specimen of the siliceous deposit taken from the side of a fissure from which steam and carbonic acid were issuing in considerable quantities had a specific gravity of 1.99, slightly scratched felspar (orthoclase), and afforded by analysis the following results :—

* *Annales des Mines*, sixième série, tom. iii. p. 424.

Water lost at 212° F.	0·48
Water lost by subsequent ignition.	6·16
Silica	92·64
Alumina	0·30
Potash	0·27
Lime, iron, and soda	traces
	<hr/> 99·85

This veinstone, which exhibited numerous parallel bands in the direction of the enclosing walls, was selected for analysis as having been most recently formed. On boiling 35·18 grains of this substance in a finely divided state during eight hours in 4 oz. of a strong solution of caustic potash, only 2·35 grains, or 6·68 per cent., of the silica present was dissolved.

A section of this substance examined under the microscope showed the reniform structure of chalcedony, together with patches of amorphous silica and small crystals of iron pyrites; but no other appearance of crystallization could be detected. This specimen contained a well-defined fluid-cavity, in which the vacuity occupied about one-tenth of its total volume. Another portion of the deposit, which, from being further removed from the sides of the fissure, must have been of older date, afforded only 4·72 per cent. of water, and distinctly showed the presence of crystalline silica. On carefully examining the great western vein before alluded to, considerable quantities of chalcedony were found; and in one of the cavities a few small perfectly defined crystals of quartz were discovered.

Alkaline Lakes.

In that portion of California lying on the east of the Sierra Nevada are Mono Lake and Owen's Lake, both considerable sheets of water highly impregnated with alkaline salts. Owen's Lake, at the southern extremity of which I was for some weeks encamped during the fall of 1865, lies in lat. 36° 20' south, long. 118° west from Greenwich, is about twenty miles in length and eight in width. Owen's Valley is a narrow basin enclosed between high mountains, and has a length of about 140 miles, and an average width of 10 miles. On its western side it is bordered by the Sierra Nevada, which opposite the lake rises in peaks fourteen thousand feet in height. On the eastern side the mountains are more broken, although of nearly equal height, but nothing like a complete exploration of this region has yet been attempted; besides which it is so utterly barren and forbidding that the Indians, whose stronghold it still remains, are not likely for some time to be much interfered with by the indefatigable mining "prospector." These mountains are bare,

and no stream of any importance flows from them into the valley, which is almost exclusively supplied by the melting of the snows which during the winter months accumulate in the upper portions of the Sierra.

This range, on the eastern side of the valley, is towards its southern extremity called the Inyo Mountains, whilst further north they are known as the White Mountains. Owen's River rises not far from the head of the San Joaquin, and near the southern extremity of the valley flows into Owen's Lake, which has no visible outlet, and whose banks are in many parts thickly coated by an alkaline incrustation. No fish inhabit its waters; but at certain seasons of the year it is resorted to by myriads of waterfowl attracted by the grub-like larvæ of a fly which deposits its eggs on the surface.

These grubs are about the length of a small grain of rice, but somewhat less in diameter, and are thrown on the lake shores by the waves in such vast quantities that they are collected by the Indians as an important article of food. In addition to these there are large quantities of minute crustacea resembling the *Artemia salina*.

Waters of Owen's Lake.—A sample of water taken January 3, 1866, from opposite Franklin's Ranch, had a specific gravity of 1.076, and on analysis afforded the following results:—

In an Imperial Gallon.

Chloride of sodium . . .	2942.05
Sulphuric acid . . .	589.12
Carbonic acid . . .	1206.80
Silicic acid . . .	55.34
Phosphoric acid . . .	15.43
Potash . . .	175.49
Soda . . .	2127.07
Organic matter . . .	16.94
	<hr/>
	7128.24

The above results may be expressed as follows:—

In an Imperial Gallon.

Chloride of sodium . . .	2942.05
Sulphate of soda . . .	956.80
Carbonate of soda . . .	2914.43
Sulphate of potash . . .	122.94
Phosphate of potash . . .	35.74
Silicate of potash . . .	139.34
Organic matter . . .	16.94
	<hr/>
	7128.24

In addition to the substances above enumerated minute traces of iodine were found, but in such small proportions that its presence could only be detected when large quantities of water were operated on.

As the samples employed for analysis had, before being brought to this country, been concentrated by boiling, it is evident a loss of carbonic acid would have taken place had any admixture of sesquicarbonate been present.

The incrustations, which at certain seasons of the year are found to the extent of hundreds of tons on the lake-shores, chiefly consist of carbonates of soda in which the proportion of sesquicarbonate is variable; and specimens were examined in which monocarbonate only was present. Besides the carbonates of soda, these deposits contain 3 per cent. of chloride of sodium and about 5 per cent. of sulphate of soda, together with a trace (0.22 per cent.) of silica.

Summary.

The following are some of the more important facts observed on a careful examination of the gold-regions:—

(1) The quartz veins of California almost uniformly exhibit evidences of having been formed by successive siliceous deposits parallel to the walls of the enclosing fissure; and when fragments of exfoliated slaty rock become enclosed, their planes are usually parallel to those of the quartz in which they are imbedded.

(2) All remuneratively productive gold veins contain notable quantities of iron pyrites and other metallic sulphides; and the association of these bodies is so constant and remarkable, that it is probably the result of some chemical action regulating the distribution of the precious metal.

(3) The auriferous quartz of the Pacific gold-fields invariably contains a certain quantity of potash together with a small amount of water which is not eliminated at a temperature of 212° F. Under the microscope it only exhibits well-defined fluid-cavities in specimens that are more than usually crystalline; and these, even in the same crystal, when heated, become full at by no means uniform temperatures.

(4) Mineral veins of evidently very recent date are met with in various localities on the Pacific coast; and some of these contain both gold and silver.

(5) In addition to the gold found in quartz veins, this metal occurs in certain bands of metamorphosed slates. The outcrops of these, like those of ordinary veins, are frequently coloured red or brown by the decomposition of pyrites.

(6) The old Tertiary river-beds constituting the deep diggings, and frequently overlain by a volcanic capping, contain large

quantities of fossil wood, and are often hardened into a compact conglomerate by a cement consisting either of finely crystallized iron pyrites or of silica. This iron pyrites encloses gold which is sometimes water-worn, and at others is in a crystalline or filiform state, showing that it has not been subjected to attrition. In Australia, pyrites replacing the woody constituents of the stems of trees found in similar positions has been shown by various chemists to contain large quantities of the precious metal. The siliceous cement of the ancient river-beds of California has sometimes, but rarely, been deposited in a crystalline form, and on examination such crystals have not been found to contain fluid-cavities presenting the usual vacuities.

(7) Mineral springs are exceedingly numerous, and their waters generally escape from the ground in a state of ebullition. These springs deposit silica, sulphur, iron pyrites, &c., whilst in some instances cinnabar is brought to the surface in large quantities by solfatara action; occasionally the deposited silica takes the form of an ordinary auriferous vein, and gold is stated on good authority to have been found in quartz so produced. At the surface the silica of such deposits contains a larger amount of water than that of the older quartz veins of the country, but appears gradually to lose it, and, although generally amorphous, is sometimes met with in a crystallized state.

(8) In the districts abounding in mineral springs are lakes of which the waters are highly alkaline, and which, in addition to carbonates and sulphate of soda, contain large quantities of chloride of sodium. These have no visible outlets, and act as vast evaporating-pans, in which the waters of the various streams flowing into them are being concentrated by the action of the sun's rays. The incrustations deposited on the shores of Owen's Lake contain a much larger proportion of carbonate of soda than the salts obtained by direct evaporation of the lake-waters.

In the present state of our knowledge, the foregoing facts would appear to lead to the following conclusions:—

(a) Quartz veins have been produced by the slow deposition from aqueous solutions of silica on the surfaces of the enclosing fissures*.

(b) From the general parallelism with its walls of the planes of any fragments of the enclosing rock which may have become imbedded in a vein, it is to be inferred that they were mechanically removed by the growth of the several layers to

* By "aqueous solution" it is not intended to convey the idea that silica has been dissolved in pure water, but rather in waters containing various acids, alkalies, salts, &c.

which they adhered, and that a subsequent deposition of quartz took place between them and the rock from which they had become detached. In this way were introduced the masses of rock known as "horses."

(c) The formation of quartz veins is often due to hydrothermal agencies, of which evidences are still to be found in the hot springs and recent metalliferous veins to be met with in various parts of the Pacific coast.

(d) From the presence of gas-cavities and the variable temperatures at which the vacuities in their fluid-cavities become filled, it may be inferred that they are the result of an intermittent action, and that the fissures were sometimes traversed by currents of hot water, whilst at others they gave off aqueous vapour or gaseous exhalations*.

This is precisely what is now taking place at Steamboat Springs, where the formation of a vein is in progress, and from which currents of boiling water are often poured forth, whilst at other times the fissures only give off currents of steam and heated gases.

(e) That gold may be deposited from the same solutions which give rise to the formation of the enclosing quartz, would appear evident from the presence of that metal in pyrites enclosed in siliceous incrustations near Borax Lake, in the semi-opal of the North Star vein &c., as well as from the fact of gold having been found in the interior of the stems of trees which in deep diggings are often converted into pyrites.

(f) The constant presence of iron pyrites in auriferous veins, and when so occurring its invariably containing a certain amount of gold, suggests the probability of this sulphide being in some way necessarily connected with the solvent by which the precious metal was held in solution. Without considerable addition to our present limited knowledge of the subject, it would be impossible to explain the exact process by which the solution of gold was effected. It has, however, been shown by Wurtz† (who has since suggested the probability of ferruginous salts being the means employed by nature for effecting the solution of gold‡) that finely divided gold is soluble in the sesquichloride of iron, and more sparingly in the sesquisulphate of that metal. It is also well known that iron pyrites sometimes results from the action of reducing agents on the sulphates of iron.

* Sorby, "On the Microscopical Structure of Crystals. § 1. *Crystals formed from solution in water*," Quarterly Journal of the Geological Society, vol. xiv. pp. 453-500.

† American Journal of Science, vol. xxvi. p. 51.

‡ Memoir read before the American Association for the Advancement of Science, August 1, 1866.

If, therefore, sulphate of iron, in a solution containing gold, should become transformed by the action of a reducing agent into pyrites, the gold, at the same time being reduced to the metallic state, would probably be found enclosed in the resulting crystals of that mineral*.

(g) The silica and other substances forming the cementing material of the ancient auriferous river-beds have probably been slowly deposited from comparatively cold solutions.

(h) The connexion existing between the decomposition of felspar by the agency of boiling springs, the existence of alkaline plains, and the formation of lakes containing various salts of soda and potash will be at once obvious to the geologist.

LIII. Chemical Notices from Foreign Journals.

By E. ATKINSON, Ph.D., F.C.S.

[Continued from vol. xxxv. p. 461.]

SCHRÖDER VAN DER KOLK published shortly before his death two articles in Poggendorff's *Annalen*, vol. cxxxi., of which the following is an abstract taken from the *Zeitschrift für Chemie*.

If a body be heated from zero to a given temperature, it takes up a certain quantity of heat which is consumed in increasing its temperature and for internal and external work. If, from the heat taken up, heat which has been transformed into external work be subtracted, the remainder represents the heat contained in the body after the process; and this quantity is called the mechanical energy of the body in this condition. Hence, in a definite condition, every body possesses a certain quantity of energy; the absolute quantity of energy is always unknown.

* From a somewhat lengthy investigation of the subject, I am induced to believe that gold invariably occurs in pyrites and other sulphides in the metallic form. Tarry matter, like that found in the solfatara at Borax Lake, has occasionally been met with in the quartz veins of California; and although the protosulphate of iron, resulting from the decomposition of the sesquisulphate, would, under certain circumstances, have the effect of precipitating gold from solution, it would probably not do so in the presence of large quantities of the persalts of that metal. A discovery, made by Mr. Dantree, of the fact that a speck of gold lying in a solution of the chloride of that metal may be increased to several times its original weight by the action of a small piece of cork introduced into the solution, is recorded by Mr. Ulrich. Mr. Wilkinson's experiments further prove that, besides gold itself, iron, copper, and arsenical pyrites, galena, blende, &c. likewise form favourable nuclei, which, if immersed in weak solutions of chloride of gold, receive a solid coating of metal by the agency of organic matter, such as a chip of wood floating in the solution.—'Notes on the Physical Geography, Geology, and Mineralogy of Victoria' (*ante cit.*), p. 44.

In the following considerations two principles are used:—

I. When mechanical work is performed an equivalent quantity of heat disappears, and conversely; the same is presupposed with regard to chemical work.

II. Heat cannot of itself pass from a warmer to a colder body.

In due connexion with this is the principle that mechanical or chemical work can of itself be spontaneously transformed into heat; but heat can never be changed into work without the simultaneous occurrence of a compensating action—for instance, when a quantity of heat passes from a higher to a lower temperature.

Most important in these determinations is the heat of combination, the determinations of which were mostly made by Favre. He expresses the numbers in thermal units, and refers them to equivalent quantities if the equivalent of hydrogen is taken = 1 gramme. The two constituents of a compound unite at a definite temperature; if, then, the compound formed be cooled to the original temperature of the constituents, the heat developed during the operation is the number given by Favre. This tells how much more energy is accumulated in the components in the original condition than in the combination in its final stage. Of course the constituents and the combinations must be in the same state of aggregation, or otherwise the latent heat must be taken into account. If t is the initial and final temperature, τ that at which combination takes place, the quantity of heat A is absorbed in raising the body from t to τ ; the heat of combination, W, is disengaged at τ , as well as the quantity of heat B if the product is cooled down from τ to t . Favre's number $F = W + B - A$ gives us the difference in energy; the real heat of combination is W. The number W gives us the chemical work consumed in the combination.

The author applies these considerations to chemical actions. Chemical combinations may be divided into two classes, according as the compounds have less or more energy than the constituents. Water may be taken as a type of the first; heat is liberated during combination and absorbed in decomposition. For the second class, of which protoxide of nitrogen is the representative, the phenomena are reversed. Moreover all bodies are decomposed at a sufficiently high temperature.

The compound gases may be formed as follows:—

(1) On being passed through a heated tube. Both kinds of the gases as above distinguished may thus be formed. In his considerations the author takes the following division as a basis.

A. The gases have less energy than their components, and are formed on simple heating.

HO . . .	29413	HBr . . .	9322
CO . . .	14838	PCl ³ . . .	94804
CO ² . . .	48480	PCl ⁵ . . .	100373
SO ² . . .	35520	As Cl ³ . . .	7183
ClH . . .	23783		

B. The gases have less energy than their constituents, and are not formed on simple heating.

NO ⁴	SH . . .	2741
NO ²	C ² H ⁴ . . .	22000
NH ³ . . .	22728	

C. The gases have more energy than their components.

NO . . .	8724	C ² N . . .	38994
ClO . . .	7370	CS ² . . .	9659
HI . . .	3606	C ⁴ H ⁴ . . .	8000

The numbers are the heats of combination according to Favre (energy of the constituents minus energy of the combination). Only the gases A are all formed on being passed through a heated tube: of the gases B and C only CS² is formed under these circumstances. The action in the heated tube may be twofold. Either heat becomes free in the formation of the compound, in which case the action excited in portions of the mass will be propagated through the entire mass, and if this takes place rapidly explosion will be the result; or the action is accompanied by an absorption of heat; the action is then restricted to the place where it is commenced, and the combination is then more complete. In decomposition, of course the reverse takes place. When, as in the case of water and carbonic acid, the compound explodes, the decomposition must be incomplete; the gases C, on the contrary, such as nitrous oxide and hypochlorous acid, must be completely decomposed, sometimes explosively, as in the case of hypochlorous acid. The latter depends on the temperature chosen, and on the velocity with which the action is propagated in the gases.

(2) Compound gases may be formed by catalytic action.

As the exciting body here remains unchanged, it can develop no energy, and can only produce changes in which no heat is absorbed. The gases C can never be formed by catalytic action; it is only possible in the case of the gases A and B.

(3) Compound gases can be formed by the action of the electrical spark.

The action of the electrical spark is essentially thermal. By sparks of adequate strength all combinations can be decomposed. If the sparks are less powerful, phenomena of combination may occur. At the same time the spark effects a change in the affi-

nity of the components. The gases A are formed by a single spark. If only a few atoms of the components are united by the electric spark, the heat which thereby becomes free is sufficient to propagate the action throughout the entire mass, and no explosion can in this case occur. The gases B can only be formed gradually by repeated passage of the spark, and only along its path. It here acts especially by alterations of affinity; the heat which becomes free in the action cannot form the compound. The gases C can also only be formed in the path of the spark; for here, besides the affinity, this must also furnish the necessary heat. Of course, in the decomposition of the gases by the electrical spark the inverse phenomena are observed. The gases which the spark can form with explosion are slowly decomposed by the electrical spark, and conversely.

(4) Compound gases can be formed by combustion.

Combustion is a chemical action which takes place with the development of heat; and this action itself develops an amount of heat sufficient to keep it up when once started. The gases C can therefore never be formed by combustion; and as, moreover, the gases B cannot be formed by simple heating, they can never result from combustion. The gases A alone can be formed by combustion, and even here those only in which the heat developed is adequate for further combustion. The same phenomena occur in the combustion of compound gases. These also can only burn with other gases, provided the heat developed is sufficient for the further combustion. The following gases containing hydrogen develop the corresponding quantities of heat for 1 equiv. $H=1$ grm., where heat absorbed is designated as negative. The second series refers to one equivalent of oxygen, or 8 grms:—

OH . . .	29413	HO . . .	29413
ClH . . .	23783	$\frac{1}{2}(CO^2)$. . .	24240
BrH . . .	9322	$\frac{1}{2}(SO^2)$. . .	17760
$\frac{1}{3}(NH^3)$. . .	7573	CO . . .	14838
$\frac{1}{4}(C^2H^4)$. . .	5500	ClO . . .	-7370
SH . . .	2741	NO . . .	-8724
$\frac{1}{4}(C^4H^4)$. . .	-2000		
IH . . .	-3606		

Each succeeding gas cannot possibly be formed by combustion from a preceding one. Of the preceding gases, only the gases A can be formed by combustion; that is, only HO, CO, CO^2 , SO^2 , ClH, BrH: water is formed by the action of O on all the above hydrogen compounds, HCl by heating Cl with the following hydrogen compounds, &c. In like manner HO is formed by heating H with all the above-named oxygen compounds; CO^2 results from heating C with SO^2 , CO, NO, &c. In this connexion some interesting cases occur: thus the reaction $2CO + S = SO^2 + C$

must be possible; the action $2\text{CO} + \text{C} = \text{CO}_2 + \text{C}$ has already been observed by Deville, who obtained carbonic acid when he passed carbonic oxide over red-hot charcoal.

The author obtains the following numbers for the oxides of nitrogen, which express in thermal units the excess of the energy of the compound over that of its constituents:—

NO	— 8724
NO ²	< 22968
NO ³	< 16354
NO ⁴	< 35468
NO ⁵	< 43623

Here NO³ and NO⁵ are assumed to be in solution. It follows from these numbers that the aqueous NO⁵ is the most stable—moreover, that NO⁴ is far more permanent than NO². Anhydrous NO⁵ has a different deportment; it dissolves in water with considerable disengagement of heat, and therefore has more energy than aqueous NO³. Hence also NO² could never form NO⁵ with oxygen except in presence of water; NO² and O can only form one product of less energy. Such a case is the NO⁴ here formed.

The sudden ignition of many bodies when heated, for instance chromic oxide, Gadolinite, Samarskite, &c., the author explains on the supposition of a sudden change in the energy. Many bodies occur in two conditions with different energy. Yellow iodide of mercury passes into red with disengagement of heat. If a body has been transformed from one state into another by heat, it stops in it, and on cooling does not again absorb heat.

Many phenomena which have hitherto been ascribed to greater or less chemical affinity, the author thinks he can explain by his theorem of mechanical energy. Thus sulphuric acid is not a stronger acid than hydrochloric because it forms a precipitate in chloride of barium and expels hydrochloric acid. A solid body is here formed under disengagement of heat. In nature a tendency prevails to transform chemical work into heat; and that may be the cause of the formation of a solid body.

M. Becquerel has published* a series of researches on the chemical effects produced in capillary spaces. The following account of them has been taken, with occasional abridgment, from a paper by Professor De la Rive in the *Bibliothèque Universelle de Genève*.

M. Becquerel's first series of experiments were executed by means of a slightly cracked tube. The cracks are made by tracing one or two longitudinal lines with a diamond on the surface of the tube, and then applying in any part of these lines the

* *Comptes Rendus*, May 13, June 17, and November 4, 1867; and January 13 and April 20, 1868.

point of a small tube heated to redness. The crack must not be allowed to become too large; it is merely necessary that it shall allow the solutions, or at any rate one of them, to enter by capillary attraction.

One of the most remarkable effects is obtained by filling such a cracked tube with a concentrated solution of nitrate of copper, then introducing this tube into a test-glass containing a solution of monosulphide of sodium which marks 12° to 15° of the areometer, the two liquids being at the same level, so that the pressure is the same on the two sides. By simply mixing the two solutions, black sulphide of copper and nitrate of soda should be formed. This, however, is by no means the case; a short time after the experiment had been arranged, a very brilliant deposit of metallic copper with a crystalline texture began to be perceived. Little by little the deposit increased, the crack enlarged, and the tube ultimately broke. Small cylinders of copper 2 millims. in diameter could then be withdrawn.

With several other solutions of copper the effects are the same, but less rapid. Similar results are also obtained with concentrated solutions of silver, lead, gold, tin, cobalt, and nickel.

It is very difficult to obtain reduction effects with those of iron and zinc.

The author used several kinds of capillary spaces besides that above mentioned—such, for instance, as the interval between two glass or quartz plates pressed more or less closely together, the porosity of the parchment-paper used for dialysis, the capillary intervals between grains of glass or of quartz, unbaked porcelain or sandstone, tubes drawn out at one end. The first method gave good results; by its means the reduction could be effected of gold, copper, lead, nickel, cobalt, and that of platinum, which could not be accomplished by using the cracked tube.

M. Becquerel in his first memoir, after having investigated the influence of the various kinds of capillary spaces, and described a great number of cases of reduction effected by electro-capillary, or rather chemico-capillary actions, cites instances of the formation of insoluble compounds by the effect of these same actions. In conclusion, he observes that there is now a whole series of facts to be added to those he has observed, such as the phenomena due to the properties of spongy platinum and of porous bodies, to the effects of diaphragms, and those of dyeing, all of which may be classed as an important part of chemistry under the name of capillary chemistry.

In his second and third memoirs M. Becquerel endeavours, while increasing the number of facts, to analyze the physical and chemical causes which come into play in the phenomena of

metallic reductions which he has described. Before examining the explanations he has given and the experimental proofs on which he rests them, it will be convenient to dwell on some facts of detail which he develops in various parts of his investigation.

Among these may first be noticed the effects produced by different kinds of diaphragms, such as bruised quartz and glass in thin layers, fine sand, and hardened mortar, the interstices of which are capillary spaces. To examine the effect of fine sand, he introduces a certain quantity into tubes closed with a piece of sackcloth bound by means of a wire on the outside, thus forming diaphragms of 4 to 5 centims. in height; he then places successively various metallic solutions in the tube, and the solution of monosulphide of sodium in the test-glass, taking care that the sand is fine enough and the height of the column sufficient to allow the liquids only to mix slowly. Most metals are thus reduced from their solutions; it is thus with copper, silver, gold, cobalt, nickel, &c. A solution of equal parts of nitrate of copper and of silver first gives the silver in dendrites or in plates; then comes the copper, but after a considerable interval. With hardened mortar the reduction of platinum, cobalt, &c. is effected. M. Becquerel has also endeavoured to replace, in his old experiments on simple electrochemical apparatus, the kaolin which he used as a diaphragm by cracks in tubes. Thus in the couple which is called the oxygen one, he substituted for the tube filled with kaolin, which separated the solution of caustic potash from nitric acid, a slit tube closed by the lamp at one end, and he obtained the same effects. The same result was obtained in other analogous cases, in which he used simple electrochemical apparatus for obtaining simple and double sulphides and iodides.

M. Becquerel thinks the reduction of metals in capillary spaces cannot be explained, as has been attempted, simply by reference to molecular attraction, but that it is due to the combined actions of affinity, molecular attraction, and electricity.

In order to show the influence of electricity in the production of the phenomenon, M. Becquerel uses the apparatus consisting of a cracked glass tube filled with a solution of nitrate of copper, which is immersed in a test-glass containing a solution of monosulphide of sodium; he then connects these two solutions by a platinum or copper wire, which by completing the circuit forms a simple electrochemical couple, which gives rise to a current which is indicated by the galvanometer. In a short time the solution of nitrate of copper is decomposed, and the end of the wire which is immersed in this solution is covered with metallic copper. During these reactions the crack only acts by making contact between the two solutions; no trace of metallic copper

is observed in the interior of the crack on the inner surface of the tube; but if the copper wire be removed, which suppresses any metallic contact, the fissure fills with small crystals of metallic copper, as well as the inside of the tube. In this case, according to M. Becquerel, the liquid lodged in the fissure acts as the copper wire which has been suppressed; and there are two currents, which add on to each other, for they are in the same direction—one due to the reaction of the liquids, the other arising from the oxidation of the reduced metal. When the fissure is filled with metal, it may happen that there is no direct contact between the two liquids, and the couple then ceases to act. The same results are obtained with other metallic solutions, such as nitrate of silver. It may be remarked that, capillary spaces of the same section being far more imperfect conductors of electricity than the metallic wires whose functions they perform, the electrochemical effects should also be feebler.

M. Becquerel assumes that liquids enclosed in capillary spaces play the part of solid conductors. This he finds supported by the circumstance that, from M. E. Becquerel's experiments, the conductivity of liquids in capillary tubes is greater than is due to the section of the liquid column. This important fact would tend to prove that the increase in density of the liquid layer retained on the surface of the glass by molecular attraction comes into play to form a voltaic circuit consisting solely of liquids.

I shall not dwell on the various improvements successively made by M. Becquerel in the construction of these apparatus, nor on the numerous experiments made to replace monosulphide of sodium by other solutions. Before concluding I will restrict myself to remarking upon a few points which appear to me more important.

I will first call attention to a series of facts which would seem to prove the production of electric currents in the fissures of cracked glass vessels without the apparent intervention of chemical actions. In all the phenomena previously described, the fissures of cracked vessels, though very narrow, nevertheless enable the two liquids which moisten the sides to react chemically upon each other in the capillary spaces and to give rise to electrochemical effects. Now observe what takes place if the magnitude of the fissures be varied. If the size is relatively great, the solutions mix more or less rapidly, giving rise to the ordinary precipitates which result from a double decomposition. With a smaller size the electro-capillary phenomena of metallic reduction which we have described are obtained. At a lower degree there is an electrical current without any apparent chemical action—a current which has the same direction as if the reaction took place. The cracks may finally be so narrow that

there is neither metallic reduction nor electrical current. In this case evidently the liquids no longer communicate.

To obtain an electric current without apparent chemical action, fissures having the shape of a star are made in a small cup by touching it with a slightly moistened glass after having heated it. The cracked beaker is then filled up to the star with a solution of nitrate of baryta, and is placed in another vessel containing weak sulphuric acid. Two platinum plates, each immersed in one of the liquids, are connected by the wire of a galvanometer; and an electric current is obtained without any appearance of chemical action; that is to say, there is no deposit of sulphate of baryta. Associating in the same manner a solution of chromate of potash with one of nitrate of lead, or a solution of ferrocyanide of potassium with a solution of protosulphate of iron, no precipitate is obtained, which proves that there is no apparent chemical action, although an electric current is produced. On the other hand, working with solutions of nitrate of copper and of monosulphide of potassium and the same vessel which served for former experiments, a strong current is obtained, and some time after there is a deposit of metallic copper in the fissure. It is true that the electromotive force which takes place in the contact of these two latter solutions is far more considerable than that of the other solutions which had been submitted to experiment. M. Becquerel remarks that it is possible that with these latter solutions, the chemical reactions being extremely feeble, an electric current is produced, though the effects resulting from the chemical action which has produced it are not perceived for a long time.

In this part of his researches M. Becquerel dwells on what he calls the polar properties of the sides of the fissures of cracked vessels, acting like electrodes when these fissures furnish the passage of a current. This is also an interesting point, on which I will delay for a moment, inasmuch as it seems to me connected with a more general law discovered by Faraday. The following is M. Becquerel's experiment.

When we decompose a solution of nitrate or sulphate of copper partly contained in a cracked vessel and partly in a sound one in which the former is immersed, and if by means of a platinum electrode placed in each of these vessels the current from a battery composed of several couples be passed, a decomposition is obtained in which the negative electrode becomes covered with metallic copper; the current, however, has but small intensity, owing to the thin layer of liquid found in the crack, which in this case plays no part. It is not the same if the cracked vessel contains a solution of caustic potash and the negative electrode while the other vessel contains the metallic solution and the positive electrode. In this case water alone is

decomposed, and not the metallic solution, so long at least as the two solutions are not mixed.

If, now, two cracked vessels be taken containing acidulated water and immersed in a solution of nitrate of copper, the sides of the fissures act in that case like intermediate conductors; the outside of the positive vessel becomes covered with metallic copper, while the acid is given off on the negative electrode. The part of the narrow fissures is here that of preventing the mixture of solutions; they act then like intermediate conductors; the two plates of platinum are in conducting non-metallic solutions, which do not mix with the metallic one, though they are in contact with it.

The phenomenon we have described is quite analogous to an experiment of Faraday, in which that illustrious physicist passes an electrical current through a concentrated solution of sulphate of magnesia in contact with a certain quantity of pure water placed above, and arranged so that the two liquids cannot mix. Putting the positive electrode in the sulphate of magnesia and the negative one in water, the magnesia, instead of being deposited on the surface of the negative platinum electrode, stops in the water at the boundary of it and the sulphate. This fact, which is easily explained on Grothüß's theory, is owing to the fact that the molecule reaching the water no longer finds a molecule of acid with which it can combine, which would not be the case if the water and the sulphate could have become mixed even in small proportion. It is probably a phenomenon of the same kind which occurs in Becquerel's experiment, and that the fact he has observed is due to the circumstance that, the solution of nitrate of copper only being connected with the two acid solutions by the intervention of the capillary fissures, there can be no mixture between them. But when there is no mixture in more or less considerable proportion between two adjacent solutions, there can be no ordinary electrolysis.

In his fourth memoir, M. Becquerel describes a very ingenious method of M. E. Becquerel of measuring capillary spaces, and especially the magnitude of fissures, and even the dimensions of the pores of vegetable or animal membranes. The method in question consists in comparing the conducting-power of a liquid enclosed in the capillary space to be measured with the conducting-power of the same liquid placed in a capillary tube, the diameter and length of which are exactly known. This method is extremely delicate, and by its means intervals of a few thousandths of a millimetre may be measured; yet it requires in its application some precautions and certain preliminary determinations.

Measurements made by this method have shown that when

the interval between the plates is only a few hundredths of a millimetre, most of the metals are reduced; while when it is only a few thousandths of a millimetre, silver and gold are easily reduced, but the others only slowly and with difficulty.

Using as a capillary space the interval between two plane glass plates kept pressed against each other by a ligature, so that there is only a very small capillary interval, the effects obtained are much more regular than with slit tubes, which have not everywhere the same width. The liquid, which is a solution of a metal, is introduced between the plates by means of a small reservoir at the top, and the supply is so arranged that the exit of the liquid by the edges is inappreciable, which is easily effected. The system of two plates between which is the liquid is then immersed in an outer vessel containing the other solution, that of the monosulphide for instance.

It sometimes happens that the metallic deposits formed between glass plates merely separated by an interval of a few hundredths of a millimetre, between which is a band of filtering-paper, and kept firmly bound, exert such an expansive force that even when the bands are not broken one of the plates is. From this it is readily seen how rocks through which various solutions infiltrate, and in which are produced capillary effects resembling those we have described, may crack or even burst.

We shall not follow M. Becquerel in the applications which he makes of his new researches to explain various natural phenomena, either in inorganic or in organic nature. It is, in fact, readily understood that, as soon as we are concerned with cases in which capillarity plays an important part, this application is a natural consequence. In inorganic nature the infiltrations which take place in the slits of rocks ought to produce effects resembling those of which we have spoken, and, especially under favourable circumstances, produce a reduction of metals. In organic nature it is easy to understand the important part which the porous tissues and narrow vessels of vegetables and animals necessarily play. M. Becquerel has endeavoured to establish relations between endosmose, exosmose, and dialysis and electro-capillarity, of which, in his opinion, the former phenomena are only particular cases.

In conclusion M. De la Rive sums up the results of these researches as follows :—

The fundamental point is the influence exerted on the phenomena which occur in the contact of the two liquids by the fact that this contact takes place through a very narrow space, in other words, a capillary one. Is this influence merely negative? that is, does it simply arise from the fact that the two liquids being separated by a narrow space cannot mix, being all the while in contact?

We think that this is sometimes the case, as, for example, in the electrolysis of two liquids placed end to end in the path of the current, so as only to communicate by the intervention of capillary spaces, and therefore without being able to mix. But, further, the phenomena of exosmose, of endosmose, and dialysis show that there may be a positive influence arising either from the fact that the liquids are only in contact by very small surfaces, or from the nature of the solid which serves as envelope for the capillary spaces.

The phenomena which M. Becquerel has observed seem also to indicate that the relative nature of the solution and of the porous solid is not without influence, and in general he attributes the effects he has perceived to the production of electricity. According to him, when the two solutions are not united by a solid conductor, a case in which obviously a couple is formed, the small layer or the liquid column contained in the capillary space acts as this solid conductor; or at all events the sides of the insulating material (generally glass) of the capillary space perform the function when they are moistened by a liquid which is itself a conductor. In this manner we should explain the reduction against the sides of those metals whose solutions are in contact with them. M. Becquerel, among other proofs in support of this explanation, mentions that, according to M. E. Becquerel's researches, the liquid columns contained in capillary spaces conduct better than corresponds to the law of diameters.

We must confess to some hesitation in admitting the preceding explanation. We shall first ask whether, in the case in question (that is, when there is a chemical action between two solutions separated from each other, either by diaphragms or by capillary spaces which enable them to be in contact without mixing), electricity does really come into play. That electricity really does intervene when the two solutions thus arranged are connected by an external metallic conductor cannot be doubted; for then a couple is formed, and the ends of the conductor are true electrodes, which receive the deposits arising from the electrolytic decomposition. But when the two solutions are merely in contact without mixing, being separated by a porous diaphragm or by a capillary space, electromotive force is undoubtedly developed in the two liquid surfaces in contact, a development followed by a chemical action, and by the production of an electrical current; but the current is in this case molecular or local, and can only be perceived when the two liquids are united by a solid conductor; it would seem to us to be rather the result than the cause of the chemical action which takes place. Instead, then, of assuming that the liquid enclosed in the capillary space acts as a solid conductor, it would seem more natural

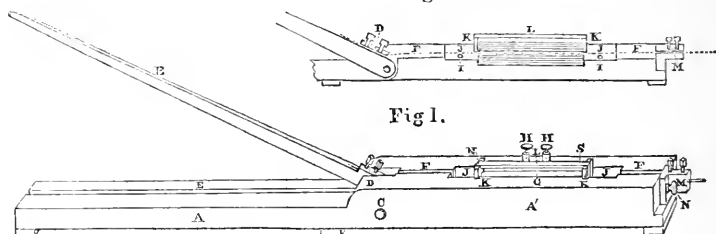
to suppose that the effect observed is only the result of the chemical action, more or less modified by the fact that the space is capillary—and in particular that the action can only take place on a small number of molecules at a time and successively, instead of acting on the whole of the two solutions at the same time. This is evidently an important point to investigate. It is very probable that the chemical action is modified by the so-called molecular attraction to which capillarity is attributed. It is also possible that this so-called molecular attraction is only one form of affinity—or rather that the two forces are only one, so that in the capillary spaces the liquid is not in the same conditions, physical or chemical, which it presents when it is in mass; thus the action of another liquid on the capillary layer would be quite different from that which it exerts in its ordinary state. It would be an action of the same kind as those of contact, the so-called capillary ones.

The greater the advance in the study of chemistry the more insufficient do we find the explanations of chemical phenomena that are merely based on the exercise of the force called affinity as hitherto understood. As I have already said, a host of circumstances demonstrate this inadequacy. One of the most remarkable of the phenomena is the modification which the presence in a liquid or a gas of an apparently inert solid brings about in the chemical relations of this gas with others. And, in fact, the limiting surface of a liquid or a gas should have different properties from the rest of the layer. Now the characteristic of liquids placed in capillary spaces is that of being exclusively formed of limiting surfaces; and therefore it is not very surprising to find that in these conditions their properties become quite different from what they are in their natural state. Considering Clausius's theoretical ideas on the constitution of liquids and gases, those of Deville on dissociation, of Graham on dialysis, and those of Bunsen on the combination of certain gases, without taking into account the older ones of Berzelius on catalysis, of Dumas and so many more on endosmose and exosmose, we come to the conclusion that we have an entirely new force of chemistry to deal with. It seems therefore premature to endeavour to give a rational explanation of the phenomena recently discovered by M. Becquerel by ascribing them to an electro-capillary effect. They are probably only one of the numerous forms of chemico-molecular actions in which the physical constitution of the body plays as important a part as its chemical constitution. And even if we rejected all M. Becquerel's theoretical ideas on this subject, this illustrious physicist would none the less have rendered a great service to science by directing the attention of men of science to these phenomena.

LIV. *On the Relation of Mechanical Strain of Iron to Magneto-Electric Induction.* By G. GORE, Esq., F.R.S.*

FOR making experiments on this subject I have constructed the following apparatus:—A A' is a strong base or plank of oak about 120 centims. long, 18 centims. wide, and 7·5 centims. thick (the thinner part of which, marked A, may with advantage be left of the same thickness as that of A'). B is an iron lever about 72 centims. long, and 1·5 centim. thick at its lower end, capable of moving in a vertical plane upon a strong horizontal rod or axis of steel C, about 2 centims. diameter, driven tightly into a hole in the wooden base; the lever has a piece of iron D (shown in section in fig. 2) welded upon its upper sur-

Fig. 2.




face, with a hole 6 millims. in diameter drilled through it for the reception of one end of the iron wire. E is a slot cut in the wood for the reception of the lever. The wire to be strained cannot be shown in fig. 1, because it lies in a narrow groove FF, about 8 millims. wide and 3 centims. deep, cut in the base-board, the wire being at a distance of about 1·5 centim. below the surface of the board; its position is shown by the horizontal dotted line in fig. 2. G is a coil of insulated copper wire; it is about 18 centims. long and 4·3 centims. in diameter, with a hollow axis of glass tubing of about 8 millims. internal diameter; it contains about 3295 turns of wire 0·54 millim. thick (= No. 26 Birmingham brass wire-gauge), the two ends of the wire being attached to the binding-screws H, H. The coil lies tightly fixed by wedges of wood in a rectangular space in the wood, with its axis coincident with that of the iron wire. The iron wire is kept in the same horizontal plane by passing over two brass rollers (without axles) I, I, shown in section in fig. 2; these rollers are about 2·5 centims. long and 1·5 centim. in diameter, and lie upon the bottom of the hollow rectangular spaces in the wood shown at J, J. K, K are two moveable rectangular pieces of oak which slide vertically and tightly in narrow grooves formed

* Communicated by the Author.

in the sides of the rectangular space in the base-board; they have fixed to them two small vertical plates or armatures of soft iron, the lower edge of each of which has a small notch filed in it which fits and *presses* upon the iron wire; and when the apparatus is going to be used, a permanent bar-magnet L is placed with its poles resting upon the upper ends of these armatures. The iron wire employed is usually about 4 millims. in diameter, and the apparatus is sufficiently powerful to readily elongate iron wire of that size. M is a rectangular or T-piece of wrought iron of the form and dimensions indicated by the figures; it is moveable, and may be readily taken off the end of the base-board by removing the screws N (of which there is one on each side). The ends of the wires require to be held very securely during the strain; this is perfectly effected by having in D and in M two steel screws each 6 millims. diameter. The level of the top surface of the small brass rollers is slightly above that of the ends of the iron wire.

By means of this apparatus I have repeatedly verified the fact that a magnetized soft iron wire, during the act of being stretched (either with temporary or permanent elongation), *increases* in magnetism, and produces a current of electricity in the coil of wire G in a contrary direction to that of the hands of a watch, *i. e.* in

the direction of , when we are looking at its south pole, cor-

responding to that of the north pole of the magnet above it. With a galvanometer of moderate sensitiveness, the amount of deflection of the needles was generally about 14 degrees on one side by a single stretch of the wire, or 17 degrees on each side by repeated synchronous stretches. No deflections were obtained by stretching thick wires of zinc or copper. The apparatus was placed in a direction of east and west during the experiments.

The results obtained with iron agree with the fact discovered by Mr. Joule, that straining an iron wire lowers its temperature, and also with the well-known fact that lowering the temperature of a piece of iron increases its magnetic capacity.

LV. *On the Bearing of Recent Observations upon Solar Physics.*
By G. JOHNSTONE STONEY, M.A., F.R.S., &c.*

97. **T**HE main difficulty in dealing with solar and stellar physics arises out of the scantiness and fragmentary character of our data. This makes the inquiry a very treacherous one—so much so, that some of the most eminent men have gone

* Communicated by the Author.

astray in attempting it, and no one that has ventured upon the enterprise can deem himself secure. For, on the one hand, that which at any stage of information appears the most obvious account of a phenomenon is often not the true one; and the meagre knowledge we have of solar phenomena seldom suggests much beyond the obvious account of them; and, on the other hand, if we are led astray on any point, we have little to set us right, as we cannot select our test; and all we can do is to lay down correct principles of interpretation, to bring to bear from every quarter such shreds of knowledge or information as can be laid under contribution, and to review all, from time to time, in the most cautious and painstaking manner.

We may therefore well hail with satisfaction the additional news of the sun which the late eclipse has brought us. These fresh items of intelligence are chiefly two:—that the light of the corona, so far as it was examined (that is, in its upper regions), is much of it polarized in planes passing through the centre of the sun; and that the spectrum of the prominences which were examined contained bright lines. It is with these particulars that I wish at present to endeavour to deal.

98. The protuberance upon which all the observers*, except Captain Haig, appear to have bent their chief attention was an unusually brilliant and lofty one on the eastern limb of the sun, nearly opposite to a solar spot†. It emitted five bright lines, and apparently four fainter ones not seen by most of the observers. Major Tennant satisfied himself that the hydrogen line C was one of the bright lines, the sodium line D another, and the magnesium line *b* a third‡. He had not time to measure more; but his estimate of the positions of the two other lines which he saw suggests their being the two other principal hydrogen lines, viz. F and the hydrogen line close to G. Lieutenant Herschel's measures identify one line with D, and place another near F§. Mr. Lockyer, examining another prominence since the eclipse, has satisfied himself that he saw as bright lines C, F, and a line near D||. M. Rayet, who records nine lines, supposed the brightest lines to be B, D, E, F, and G, "judging

* There is some uncertainty whether this was the prominence examined by Major Tennant, since he speaks of the prominence he examined as being on the preceding, *i. e.* the west, side of the sun. But his description of its aspect does not seem to accord with the appearance of any other than the great flame seen on the eastern limb, and he refers to its being conspicuous immediately before the sun reappeared as evidence of its great height, which also seems to imply that it was on the eastern side.

† Proceedings of the Royal Society, vol. xvii. p. 79.

‡ Monthly Notices of the Astronomical Society, vol. xxviii. p. 245.

§ See 'The Engineer' of November 6, 1868, p. 346.

|| Proceedings of the Royal Society, vol. xvii. p. 91.

from their arrangement in the field of view, the intervals between them, their colour, and general aspect"*. To bring this into harmony with the preceding determinations in which measures were made, we must suppose that M. Rayet's B and E were in reality C and *b*. It is to be observed that the intervals between B and C, and between E and *b*, are the two smallest of the intervals between lines lettered by Fraunhofer, and therefore that these rays are the most liable to be confounded in such eye-estimates as M. Rayet made. The details of M. Janssen's observations have not reached Europe; but he describes hydrogen as detected in the principal protuberance; and therefore his report also inclines in favour of C. On the whole, then, we may with most safety conclude that the lines seen were probably C, D, *b*, F, the hydrogen line near G, and four other very faint lines. Those which seem to be identified are the principal lines of hydrogen, sodium, and magnesium—the three gases in the solar atmosphere which, as I have elsewhere shown†, project far beyond all other known constituents.

99. The most probable account to be given of this great prominence would appear to be that which I ventured to advance in a paper which was presented a year and a half ago to the Royal Society (see Proceedings of the Royal Society, vol. xvii.). [For convenience of reference the paragraphs of this paper are numbered in succession after those of the paper in the Proceedings of the Royal Society.] There is a low-lying shell of attenuated clouds outside the photosphere, at a distance from it of somewhat more than one semidiameter of the earth. Now, in the memoir already referred to, I have shown (§ 68) that if a disturbance in the lower parts of the outer atmosphere heave a part of this shell of cloud above its natural level into a cooler region, it will, from its continuing to be exposed to the unmitigated glare of the photosphere, import an enormous amount of heat into its new neighbourhood, which will have the effect of so heating the air through which the mist is dispersed, that the entire mass of cloudy particles and interposed gases will commence a violent ascent, and thus both produce a cyclone over the photosphere which may result in a spot, and give rise above to such great columnar flames as the one which has been found to give out these bright lines. When writing my paper I did not overlook the circumstance that the intermingled gases would emit bright lines; but I was under the impression that all such lines would be extinguished by the absorption of the surrounding colder parts of the atmosphere through which they should make their way

* *Comptes Rendus*, October 12, 1868, p. 758.

† See memoir on the "Physical Constitution of the Sun and Stars," *passim*, Proceedings of the Royal Society, vol. xvii.

before we could be spectators of them. It now appears that some, at least, of them escape—or, to speak more accurately, the marginal parts of some of them; for it will probably be found, if they can be submitted to examination with a sufficiently fine slit, that there is a central stripe of each blotted out by absorption, and appearing as a narrow dark line in the middle of each bright line. We must also bear in mind that there may be other lines emitted so narrow that they cannot cross the surrounding atmosphere.

100. Which lines then reached the earth? Plainly those which, as they were emitted by the incandescent gas, were broader than the corresponding dark lines of the solar spectrum, or at least than that part of the width of the latter which is due to the absorption of those strata of the sun's atmosphere through which we have to look in viewing the prominence. And as even lines of the same gas differ from one another in this tendency to spread out when heated, we see at once how one hydrogen line, C, could seem to Mr. Lockyer to extend further downwards in the solar atmosphere than another, F*. The lower part of F was no doubt effaced by the greater absorption of a hotter part of the atmosphere telling with more effect upon F than upon C. We must remember, too, that the ascending column may carry with it gases from lower regions of the solar atmosphere to heights not usually reached by them. Any rays which they would then emit while in the gaseous condition would be unobstructed in passing out through the surrounding atmosphere; and such may be some of the faint lines seen by M. Rayet. However, such constituents would probably, for the most part, soon cease to be gaseous, and augment the cloud by being chilled into a mist, or by hanging on the limits between their precipitated and gaseous conditions in the way explained in § 71. In any of these ways their spectral lines may be either enfeebled or suppressed.

101. Solar clouds may be of various kinds (see §§ 69 and 71). But the light from a sufficiently attenuated mist of particles giving a continuous spectrum would have the property of being so weakened in the spectroscope by being spread over the whole length of the spectrum, that it might be easily overlooked in such hurried observations as were made last August†. It appears probable, therefore, that the protuberance which was most

* *Comptes Rendus*, October 26, 1868, p. 838.

† Thus if a salt of sodium be burned in a candle-flame, though the sodium adds but little to the aggregate light of the flame, yet in a spectroscope with a sufficiently narrow slit the flame will give a background of almost insensible faintness upon which the two sodium lines are brilliantly rejected.

examined consisted of such a mist giving a diffuse and inconspicuous spectrum, and of the intensely heated air entangled in it giving rise to a spectrum of bright lines, nine at least of which partially escaped absorption and reached the earth.

102. The particles of cloud in this case act as the principal carriers of heat, receiving it by radiation from the photosphere, and communicating it by contact to the interspersed air; but they are not without aid in discharging this office. In fact wherever a bright spectral line is broader than the corresponding absorption line of the ordinary solar spectrum, we may be sure that the gas emitting it is exposed to direct radiations from the photosphere of those wave-lengths which correspond to the projecting portions.

103. Hitherto we have considered only the immense columnar flame to the east of the sun, with a great spot close to its base* which was probably caused by it (§ 72). But other spectroscopic observations of much interest were made. Captain Haig† saw a red line and a yellow line (probably C and D) in the spectra of two broad protuberances on the western limb. Fortunately the slit of his spectroscope was parallel to the sun's sedge, the objects under examination happened to be opposite the point where the sun reappeared, and Captain Haig looked also at light coming from a situation close to the disk. Just before the sun's emergence he saw "a brilliant wide spectrum," in which the lines of the prominences were lost and two other bright lines presented themselves. This appears to be the only spectroscopic observation recorded of light taken from a stratum low enough in the sun's atmosphere to give a hope of seeing bright lines in the corona. But from other observations it appears likely that a rich harvest remains for other observers in this direction. MM. Stephan and Tisserand‡ saw an arch of light bordering the moon immediately after the beginning of totality, so bright that the moment when the total eclipse began could not be decided. Governor Hennessy§ saw the lower shell of clouds before the close of the total phase as an arch running round one-fourth of the circle: about twelve seconds after this he saw "a rough edge of brilliant white light," probably glancing through valleys on the moon, and in another second the sun had burst forth.

104. From these observations it appears certain that the brilliant wide spectrum which superseded the two narrow spectra of protuberances in Captain Haig's observations was the spectrum of the "shell of clouds" (§ 66) and the region between it

* Proceedings of the Royal Society, vol. xvii. p. 79. See also p. 82.

† Ibid. p. 74.

‡ See 'The Engineer' of November 13, 1868, p. 371.

§ Proceedings of the Royal Society, vol. xvii. p. 86.

and the photosphere. There can be little doubt now that this shell of clouds, which I erroneously supposed lay almost in contact with the hot stratum over the photosphere, is in reality no other than the long-sought boundary of the atmospheres of iron and of the other gases near it in vapour-density, and that the spectrum seen by Captain Haig will, when examined through a sufficiently fine slit, prove to be that spectrum of bright lines coincident with the fainter of the dark lines of ordinary sunlight, to which I last year took the liberty of asking the attention of the observers*. It is a matter equally of interest and importance to ascertain whether this bright spectrum may not be observed in the absence of an eclipse, in the way in which the spectra of protuberances have been made out by Messrs. Lockyer and Janssen †.

105. The whole of the light, however, which is seen with the telescope in this bright region cannot emanate from these spectral lines, although most of what is seen with the spectroscope probably does. For light from this class of spectral lines would maintain its brightness up to the edge of the solar disk, whereas the observations of 1860 point to the existence also of a *shell* of luminous matter (see latter part of § 66). Now, when we bear in mind what is said in § 64, I think we may presume that iron and several of the metals grouped about it in vapour-density (viz. the least volatile in an atmosphere of hydrogen of the metals calcium, chromium, manganese, nickel, cobalt, copper, zinc, and no doubt also barium, §§ 49 & 82) behave in the way indicated in that paragraph:—that their natural level in the sun's atmosphere is higher than the level they actually attain; that they are constantly making their way upwards, and as constantly precipitated by the increasing cold in a mist of liquid or solid particles which constitutes a shell of cloud conspicuous through the telescope and in photographs, but which will present with the spectroscope the appearance of a faint groundwork upon which bright lines from the subjacent gases will be seen] projected. The hot space within is filled with a mixture of all the constituents‡ of the sun's atmosphere; but throughout the enormous

* See a note published in the Monthly Notices of the Astronomical Society for December 13, 1867, and in Phil. Mag. S. 4, vol. xxxiv. p. 502.

† Proceedings of the Royal Society, vol. xvii. p. 91; and *Comptes Rendus* of October 26, 1868, pp. 836 & 838.

‡ The narrowness of the sodium and hydrogen lines in the solar spectrum (see footnote to § 39) would almost tempt one to conjecture that the stratum of cloud is the lower limit of the free hydrogen and sodium, if such a thing be possible, as well as the upper limit of the iron group of metals, and that the hydrogen and sodium, according as they diffuse downwards, either enter into some kind of union with metals or in some other way become so modified that they do not emit their usual spectra. Silver at a

range of cool atmosphere which extends outwards, in which the protuberances manifest themselves, and which are visible by borrowed light, three only of the known constituents of the sun (hydrogen, sodium, and magnesium) appear to be able to maintain a permanent footing in sensible quantities, although traces of the more volatile of the others* must no doubt also spread to nearly their full natural height, which is perhaps not far outside the gauze-like envelope of cloud.

106. If, now, a tempest sweeping through the sun's atmosphere hurry a portion of the vapours bounded by the shell of clouds into a much higher and cooler situation, they will there act the part described in § 68, and the cyclone beneath and the flaming column above will come into existence.

107. The discovery of bright lines in the spectra of solar flames is valuable, too, from its suggesting to us a possible, and, indeed, what is now the probable, cause of the four bright hydrogen lines in γ Cassiopeiæ, and the several other stars of which it is a type. If there be in these stars a shell of attenuated

high temperature absorbs oxygen, which is disengaged again as the temperature falls. Can it be that the hydrogen and sodium which are constantly streaming downwards unite in some such way with other metallic vapours, and that the compounds as they diffuse upwards are decomposed in equal quantities—the other metals to be precipitated as a mist of solid or liquid particles, the hydrogen and sodium to be set free as gases?

This hypothetical account of the limited breadth of the sodium and hydrogen lines is perhaps too much of a guess to have much prospect of being true; but I mention it here chiefly because it appears in some degree susceptible of verification. For if the atmospheres of hydrogen and sodium come thus to an abrupt end in a situation at which the temperature suddenly increases, we should expect to find them emit visible bright lines where they are heated by contact with the metallic atmospheres, though possibly much fainter lines than those of protuberances. Captain Haig's observation, that the lines of the prominences were lost when the spectrum widened out, is, so far as it goes, an answer in the negative; so are also the observations of spots in which no bright lines have yet been seen; but these answers do not seem decisive, and it would perhaps be worth endeavouring to ascertain at the eclipse of next year whether faint lines may not be seen in a slit of light from the shell of clouds kept separate from the light of regions further down.

* Such as copper and zinc. Dr. Miller in his 'Chemistry' records a curious instance of the volatility of copper at low temperatures. A beam which for many years was suspended over a furnace in a copper-smelting house in Norway contained minute beads of metallic copper studded through its texture: the copper must have been raised in vapour and so deposited within its fibres. Gold has been found similarly studding the beams of refineries. (Miller's 'Chemistry,' third edition, § 523). May we not further presume that those metals which, when rubbed, emit a smell are in some slight degree volatile in cold hydrogen? Rubbing the metal seems to remove a film of oxide and allow the moisture of the air to oxidize the fresh surface. The hydrogen which is then set free is probably what carries the taint of metal with it.

cloud permanently established at a sufficient height in their atmospheres, the cloud must act as a carrier of heat and raise the layer of hydrogen in which it floats to incandescence, while the adjoining strata on both sides remain cool. The cloud might be of any of the kinds enumerated in §§ 69 & 71 of my paper "On the Constitution of the Sun." And if the stellar atmosphere extend far outside the stratum of cloud, we may expect to find a dark absorption-band in the middle of each of the four bright hydrogen lines of these stars.

Appendix.

I would wish to take this opportunity of asking the reader to correct two errors in my previous papers. In a paper "On the Internal Motions of Gases compared with the Motions of Waves of Light," *Phil. Mag.* for August 1868, in the last line of § 2, for 16^2 or upwards of 250 times read $\sqrt{16}$ times. And in the paper "On the Physical Constitution of the Sun and Stars" cancel the footnote to § 28 (*Proceedings of the Royal Society*, vol. xvii. pp. 20 & 21).

From a review of the additional iron lines seen by Ångström with a continuous electric current, his account of which* I have succeeded in procuring within the last month, it is manifest that the cause of their not being seen in the spark from Ruhmkorff's machine is in most cases not what I conjectured in the footnote, but a purely physiological phenomenon. The spark is so evanescent (lasting only one three-thousandth, or thereabouts, of the time required to make the maximum impression on the retina) that no lines which are not of the exalted intensity that will compensate for this can be seen. It is evident, therefore, that a vastly feebler continuous current may reveal multitudes of lines not visible to our eyes in the spark.

LVI. *On the Action of Sodium on Valerianic Ether.* By J. ALFRED WANKLYN, *Professor of Chemistry in the London Institution*†.

THE ethers of the fatty acids were among the very first organic compounds to be submitted to the action of potassium and sodium. Löwig and Weidmann's paper describing researches on the action of the alkali-metals on these ethers was published in the year 1840. The results arrived at, although

* On the Fraunhofer Lines, together with a diagram of the violet part of the Spectrum, by A. J. Ångström and R. Thalen. Published by W. Schultz of Upsala.

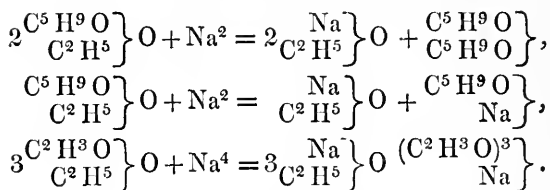
† Communicated by the Author.

for the most part correct as far as they went, were little appreciated at the time; and the subject was neglected until a few years ago, when it was taken up by Geuther, Greiner, Brandes, Frankland, Duppa, and myself.

One result has come out with the greatest clearness, and about it there has been absolute accord among all of us from the very first, viz. there is no elimination of the alcohol-radical. Neither acetate nor formiate nor valerianate of ethyl yields ethyl by the action of sodium upon it. Chloride, bromide, and iodide of ethyl, on the other hand, yield the radical ethyl (or hydride of ethyl and olefiant gas, which together represent ethyl) by treatment with potassium and sodium. In view of this fundamental difference between these two classes of ethers, and bearing in mind that in mineral chemistry no such distinction between the behaviour of the different classes of salts of a metal is to be found, I proposed in 1864 to regard the ethers of the fatty acids as salts in which not the alcohol-forming, but the acid-forming radical played the part of the metal. Acetate of ethyl I proposed to regard as ethylate of acetyl. This view, which has as yet not found much favour with chemists, is, as was shown in my former paper, in accordance with most of the known chemical history of these ethers (as, for instance, with the direction taken by the action of ammonia on them), and is in itself rational. *A priori*, we should be inclined to pronounce it more comprehensible that C^2H^3O , an already oxidized substance, should cling with less pertinacity to oxygen than C^2H^5 should do. In other words, we should expect acetic ether to split up during reaction into acetyl and peroxide of ethyl rather than into peroxide of acetyl and ethyl.

The researches of the chemists just mentioned, although agreeing with mine as to non-evolution of ethyl, differ from mine in many fundamental particulars. Their various papers, which are scattered through the chemical journals of the last five years, show that the action of the alkali-metals was regarded as being of a most complex order. Referring to Frankland and Duppa's memoir in the Philosophical Transactions, it will be seen that they regard acetic ether as polymerizing itself in presence of sodium, and then being attacked by sodium, and giving off hydrogen and alcohol, one equivalent of hydrogen for every equivalent of sodium that takes part in the reaction. And at the same time that this is going on, Frankland and Duppa consider that another portion of acetic ether deports itself quite otherwise, not polymerizing itself, but suffering a direct replacement of more or less of its hydrogen by the alkali-metal.

According to me, on the other hand, the reactions of sodium on these ethers are always simple, at any rate in the first stage:—



These are the equations by which the action of sodium on ethers of the fatty acids has been explained by me. All of them represent change of the same kind, viz. displacement of the acid-forming radical by sodium. The difference between them relates to whether the acid-forming radical is simply isolated, or enters into combination with more or less sodium.

The first of these equations was given in 1864*. The second was announced last year at the Meeting of the British Association in Dundee, and is to be found in 'The Laboratory,' p. 409. The third was also given in a modified form in Dundee, but distinctly brought forward and insisted upon at the last Meeting of the British Association at Norwich. A report of the paper read at Norwich occurs in the 'Chemical News,' 11 Sept. 1868, from which it may be learnt that one of my objections to the theory of Geuther and Frankland and Duppa is, that the equivalent of free hydrogen which they describe as complementary to every equivalent of sodium used up is mythical.

On the present occasion I have to describe particularly the research on valerianic ether wherein sodium-valeryl was obtained.

From the circumstance that so many chemists have worked at this subject with such discordant results, some idea may be formed of the difficulties of the investigation. It is essential to start with pure ethers. The aid derivable from quantitative determinations of the yield of product is required in order to make a correct interpretation of the reaction. In the instance of valerianic ether, and, indeed, in every case except that of formic, acetic, and propionic ether, there is a further source of perplexity and complication in the circumstance that there are isomers of the fatty acids, and consequently of their ethers.

Passing on to the description of my experiments:—

The valerianic ether was titrated, using alcoholic solution of potash, as has been described by me on another occasion. 100 parts of the ether neutralized 30.15 parts of potassium. The theory for pure valerianic ether requires 30.08. The valerianic ether was neutral to test-paper; and its purity may be looked upon as sufficiently guaranteed by this titration.

* Journal of the Chemical Society, vol. ii. p. 376.

In one experiment I employed—

Valerianic ether . . .	30.4 grms.
Sodium	5.8 „
Pure common ether . .	70 cub. centims

The sodium dissolved completely, without evolution of gas. To the product of the action water was afterwards added; there was again no evolution of gas. Two layers of liquid resulted on adding water, viz. an oily layer, containing, of course, much ether, and an aqueous layer, which was powerfully alkaline.

The aqueous layer was examined. The amount of free alkali ascertained by the use of test-acid was equivalent to 5.13 grms. of sodium. Furthermore, by careful distillation of the aqueous layer, there was extracted from it 4.5 grms. of absolute alcohol.

The oily layer, after being deprived of ether and dried, weighed 19 grms. A sojourn of forty hours *in vacuo* over sulphuric acid brought about a loss of 0.9 gm. A combustion made of the oil yielded the following numbers—C=66.31 per cent., H=11.7 per cent.; from which it is perfectly clear that it was not the same product as had been obtained by me in 1864 by prolonging the action, and by the employment of a higher temperature during the reaction between the sodium and the ether. The numbers obtained from an oil prepared under different conditions in 1864 were C=71.48 per cent., and H=11.67 per cent.

A further quantity of the oil having been prepared, a fractionation was undertaken. At first the liquid began to distil at about 150° C., and distilled almost to dryness at about 200° C.; but by repeated fractionation it was at length separated into two sharply defined fractions about equal in quantity.

The *low fraction* boiled between 130° and 140°, and was shown by titration and “limited oxidation” to be valerianic ether.

Titrated.—100 parts saturated 29.36 parts of potassium. The theory for pure valerianic ether demands that 100 parts shall saturate 30.08 parts of potassium.

“*Limited oxidation.*”—Treated by the well-known method of Chapman, Thorp, and Smith, it yielded only acetic and valerianic acids in equivalent proportion. Percentage of barium in the mixed baryta-salt = 46.11. The theory for an equivalent mixture of acetate and valerianate of baryta is Ba=46.13 per cent.

The *upper fraction* boiled at about 215° C.; only traces of liquid of still higher boiling-point were present, and these were removed. Combustions of it gave results in accordance with the formula C⁵H¹⁰O, and the liquid is a polymer of hydride of valeryl (valeral). It will be remarked that it is isomeric with valerianate of amyl. It differs from that ether by having a

higher boiling-point, and by not being attackable by alcoholic solution of potash, which very readily attacks valerianate of amyl. (I have titrated valerianate of amyl on other occasions, and found no difficulty in effecting the decomposition by means of alcoholic potash.)

The result, therefore, of the fractionation of the oily product was, that it consisted of about equal parts of unaltered valerianic ether and new product. Taking advantage of the unalterability of the new product by alcoholic potash and the easy decomposition of valerianic ether by this reagent, I made a titration of the original oily product in order to ascertain the relative proportions of valerianic ether and new product. 100 parts of the oil saturated 14·8 parts of potassium. The theory for pure valerianate of ethyl being 30·08, it follows that about half of the oil consisted of that ether—a result which confirms the conclusion drawn from the fractionation.

Collecting the experimental results we have:—

	grms.
Valerianic ether employed	= 30·4
Sodium used up . . .	= 5·8
Sodium found caustic .	= 5·13

The quantities required by theory in order to give one molecule of valerianic ether and one equivalent of sodium are—

	grms.
Valerianic ether . . .	30·4
Sodium used up . . .	5·38
Sodium caustic . . .	5·38

The experiment therefore accords sufficiently with this theory. As many molecules of valerianic ether are employed as equivalents of sodium, and all the sodium is ultimately caustic.

19 grms. of oil, which on prolonged exposure *in vacuo* became 18·1 grms., were yielded by these materials. This oil consisted of 9 grms. of polyvaleral ($nC^5H^{10}O$), the rest being unchanged valerianic ether. Of course there must have been much loss of valerianic ether by the evaporation off of the 70 cubic centims. of common ether; so the unaltered valerianic ether experimentally found must have been considerably less than that actually present.

Furthermore, about half of the ethyl which was taken in the form of valerianic ether was afterwards found in the form of alcohol associated with the caustic soda.

These experimental data prove the following equation to be a correct expression of the reaction between sodium and valerianic ether at a gentle heat, and in presence of much common ether.

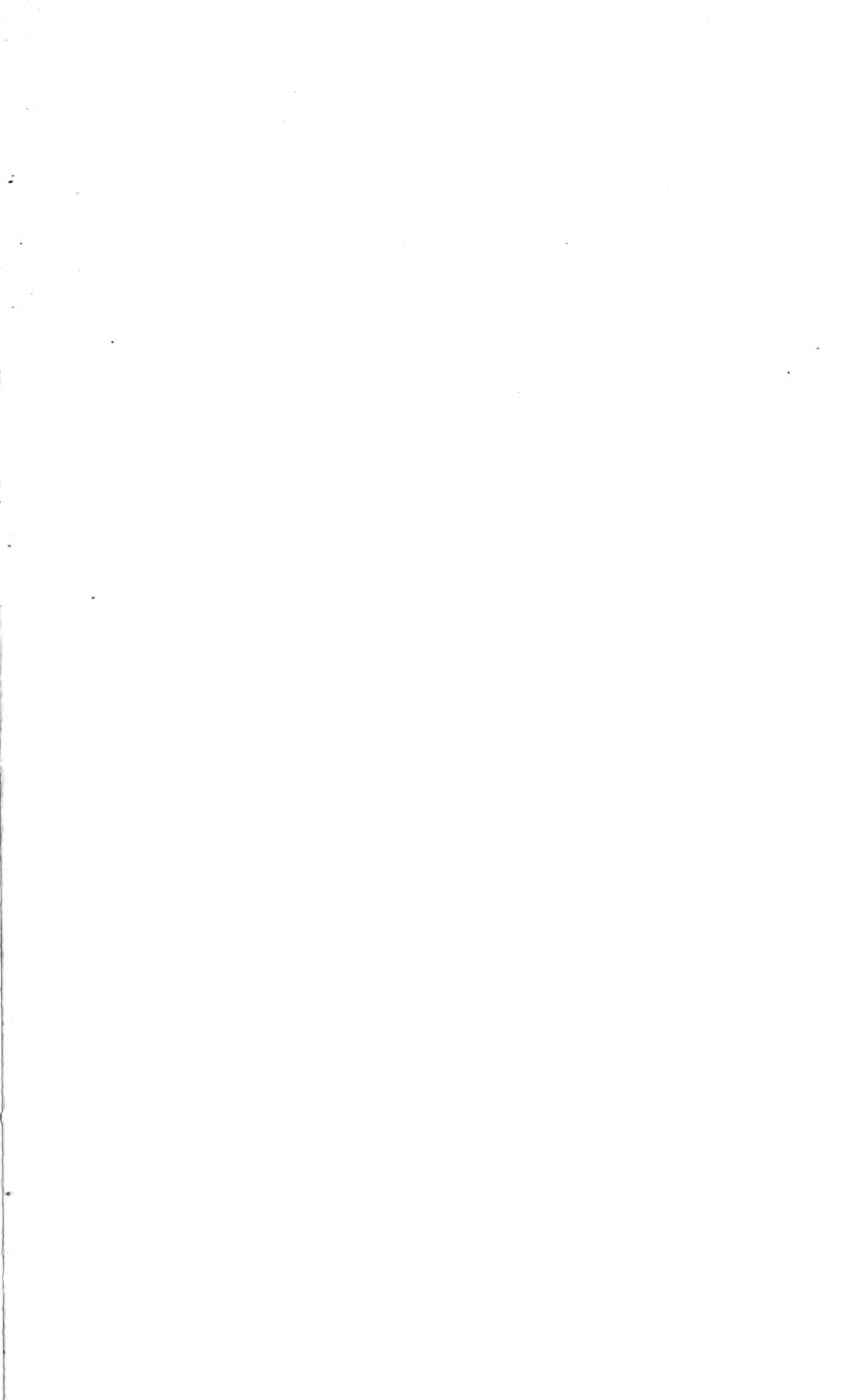


Fig. 1

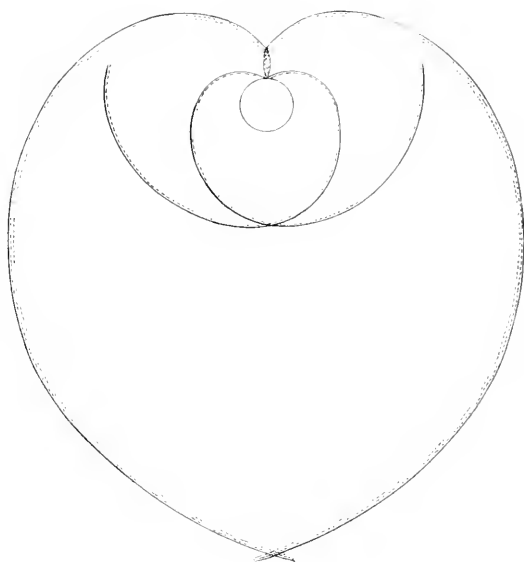


Fig. 2.

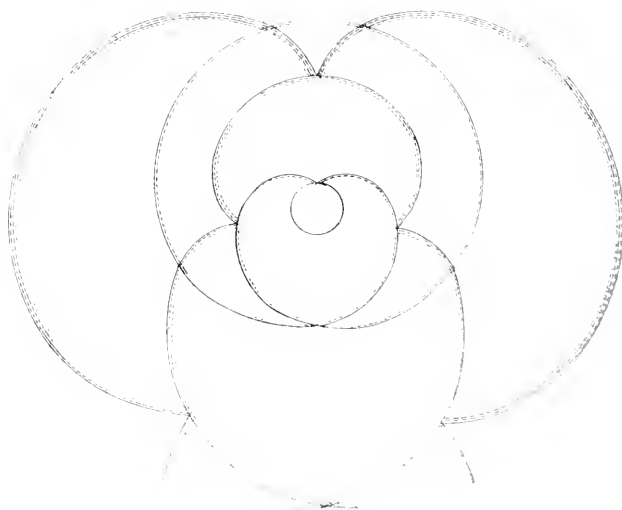


Fig 4.

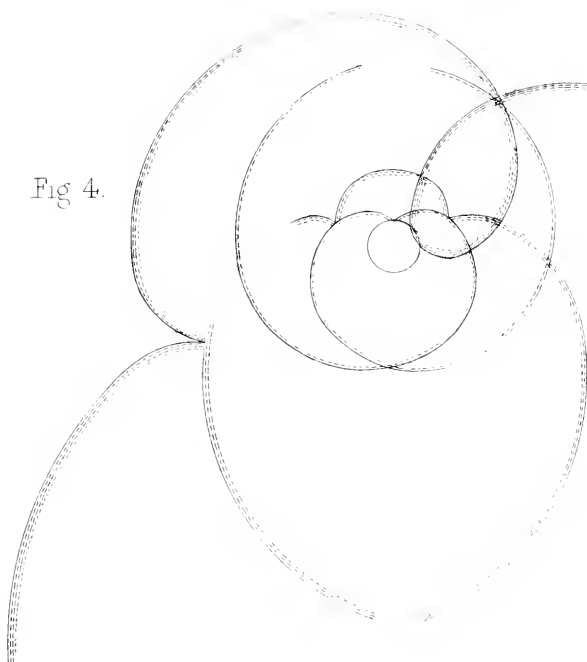
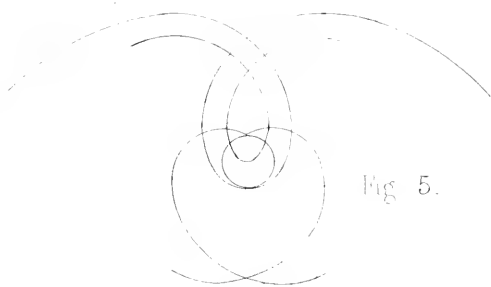


Fig 5.



1. Triple tangent
2. Double tangent passes through cusp
3. Double tangents coincide

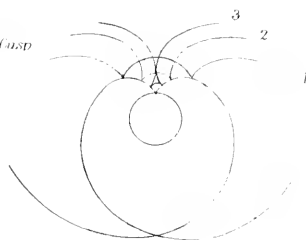
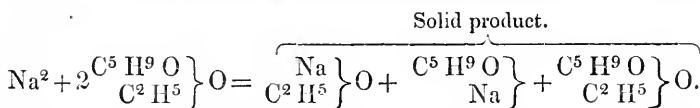


Fig 6.



On the addition of water to the solid product we should have caustic soda, alcohol, polyvaleral, and valerianic ether, in the proportions which the above-described experiments have indicated.

Polyvaleral . . . $n\text{C}^5\text{H}^{10}\text{O}$.

The properties of this very interesting compound will be more fully described on a future occasion. It is an oily liquid of a very pale yellow colour (whether this colour is essential or not I cannot tell). Boiling-point about 215°C . It has a very powerful and very peculiar smell. "Limited oxidation" appears to give only valerianic acid. Its spec. grav. in the liquid state is about 0.90 at ordinary temperatures.

London Institution,
November 1868.

LVII. *On Successive Involutes to Circles*.—Second Note*.

By J. J. SYLVESTER†.

[With a Plate.]

SINCE the appearance of the former Note on this subject, I have enjoyed the inestimable advantage of securing the cooperation of my all-accomplished and omni-capable friend Mr. Spottiswoode, to whose kindness and skill my readers are indebted for the beautiful figures given in the annexed Plate, which I shall proceed briefly to describe, and which, as far as I can learn, offer the first examples of the actual visible representation of any derived involutes of the circle beyond those of the first order. I propose, for want of a better word, provisionally to give the name of *Cyclodes* (suggested by Professor Cayley) to these spirals. They may be considered a genus of a more general class of spirals which I propose to name *algebraical spirals*, defined by the condition that the perpendicular on the tangent from a certain fixed point (which may be termed its pole) is a rational algebraical function of the angle of contingency; so that a cy-

* The thought foreshadowed in the concluding paragraph of the former note leads to the following theorem.

Let f, ϕ, ψ be quantics in α, β ; F the unicursal function obtained by elimination of α, β between

$$x=f, \quad y=\phi, \quad z=\psi;$$

$\Delta_x.F$ the discriminant of F regarded as a quantic in x and 1; $J(\phi, \psi)$ the Jacobian of ϕ, ψ ; R the result of eliminating ϕ, ψ between

$$y=\phi, \quad z=\psi, \quad J(\phi, \psi)=0;$$

Q the product of all the homogeneous linear functions of y, z which vanish at the double points of F ; then I say (and the proof is all but self-evident)

$$\Delta_x.F = R \cdot Q^2.$$

† Communicated by the Author.

clode may be said to be an *integral* algebraical spiral, *i. e.* one in which the perpendicular on the tangent becomes a rational integral function of the angle of contingence.

I find in a certain question, presently to be alluded to, the theory of the class so indisputably bound up with that of the genus, as to persuade me of the importance of the theory of the former being gone into by some one who has leisure for the investigation, and of the desirableness of an organic description being discovered or devised for the rational fractional case. The peculiar feature of the cyclode class is the absence of points of inflection, real or imaginary. The cusps of cyclodes are strictly analogous to the asymptotes in algebraical curves, like them entering and disappearing in pairs, creating partial interruptions of continuity, and thus separating the curve into distinct branches*. In the same way as the order of an algebraical curve is determined by the number of its intersections with any right line, so that of any such spiral may be characterized by half the number of its intersections with any circle having its centre at the pole. When the rational fraction which expresses the value of the perpendicular is of the degree m in the numerator and n in the denominator, the order will thus become the *dominant* of the two quantities $m+n$, $2n$.

Plate III. figs. 1, 2, 4, 5, exhibit examples of cyclodes of the first, second, and third orders, distinguished respectively, where required, by the number of accompanying dotted lines†. Let us consider more closely those of the second order, which separate themselves into two classes, the cusped and uncusped. The cusped class are the analogues of the hyperbola, the uncusped class of the ellipse, and the very remarkable secondary cyclode whose tail (to use the late Dr. Whewell's expression) is zero, and which may be termed the natural one of the order, is the analogue of the parabola. In the former Note I spoke of the point where it meets the cusp of the parent curve as an *abortive loop* or a coincident pair of cusps; the words marked in italics are calculated to convey a false impression, and are to be considered cancelled. So, too, the passage (p. 301) commencing with the words "at such points," and ending with the words "points of retrocession," and containing the misprint "will change its *curve*" for "change its course," is erroneous and is also withdrawn. Instead of points of retrocession, I propose to call these "points of radiation" or "radiant-points;" the intervention of the cusps prevents the happening of the supposed "retrocession" at such points. This error illustrates the danger of, so to say, fighting in the dark, *i. e.*

* Parallelism for cyclodes bears some analogy to projection for algebraic curves, and operates in the way of addition or diminution upon the cusps as the latter process does upon the asymptotes.

† In figs. 5 and 3, which refer to cyclodes of the second order exclusively, it has not been thought necessary to adjoin the dotted lines.

reasoning from general mental impressions in the absence of all suggestive visible representation of geometrical forms*.

Let us compare the cusped cycloides of the second order in figs. 2, 4, where the *tail* † is negative, with the *natural* one in fig. 1, for which this tail is zero. It is interesting and instructive to trace the passage from the one to the other by following the fortunes of the double tangents, which in the figures 2 and 4 will be seen to connect the sort of Moorish arch that constitutes the middle finite branch with each of the adjoining infinite branches.

How is either of such double tangents to be determined? At the two points where it meets the curve the angle of contingency is not the same, but has increased by 180° in passing from the one to the other. Accordingly p , the perpendicular, if represented by $F\phi$ when the double tangent is regarded as a tangent at one point, will be represented by $-F(\phi + \pi)$ when that line is regarded as a tangent at the other point of contact, and the equation for finding ϕ becomes $F\phi + F(\phi + \pi) = 0$. Thus we see incidentally that p must become zero, *i. e.* that a point of radiation must necessarily exist somewhere between the two points of contact. And here I may remark incidentally that this throws light on the notable equation, applicable to any curves whatever,

$$\frac{ds}{d\phi} = p + \frac{d^2p}{d\phi^2};$$

for $\frac{d^2p}{d\phi^2}$, by virtue of the remark made in a footnote to the former paper on this subject, is the perpendicular to a tangent to the second evolute at a point corresponding to that, in the curve itself, for which p is the perpendicular to the tangent; but at corresponding points in a curve and its second evolute, the tangents, although parallel in *direction*, are opposite in *flow*. Hence $p + \frac{d^2p}{d\phi^2}$ (and not $p - \frac{d^2p}{d\phi^2}$) is the distance between these two tangents; and it is obvious that such distance is identical with the radius of curvature corresponding to the perpendicular p ; so that, viewed in this light, the differential equation above written is reduced to a *truism*. Returning to our cycloide of the second

* I believe I am correct in saying that in like manner a mistake made by Steiner in his description of a surface viewed only by himself "in the depths of his inner consciousness," was first discovered by Professor Kummer after the construction of an actual model. So impossible is it to *prove demonstration*, and to make oneself absolutely safe against the fallacy of ignoring entities on the one hand, or unduly assuming their existence on the other.

† In general the *tail* is the distance of the cusp of the first involute from the corresponding points of the involutes successively engendered therefrom.

order, we may write its equation under the form

$$p = \frac{a}{1.2} (\phi^2 - \gamma),$$

where a is the radius of the base-circles, ϕ is zero at the apse, *i. e.* the point which divides the curve into two equal and symmetrical branches. Hence to find the nearest double tangent* we have

$$\phi^2 - \gamma + (\phi + \pi)^2 - \gamma = 0.$$

Putting $\phi + \frac{\pi}{2} = \psi$, this equation becomes

$$\psi^2 = \gamma - \frac{\pi^2}{4}.$$

The *tail* of the secondary cyclode (say τ) is obviously the distance of the apse (in respect of the centre) from the node of the parent first cyclode; and its length is $\frac{a}{2} (\gamma - 2)$, the distance of the apse from the centre being $\frac{a}{2} \gamma$. As γ increases towards 2, ψ decreases; that is, either double tangent tends more and more towards the horizontal; the Moorish arch therefore sinks, and the adjoining haunches rise until when $\phi = 0$, *i. e.* $\gamma = \frac{\pi^2}{2}$, the two double tangents are in direct opposition and merge into a triple tangent touching the arch at its centre.

As γ continues to decrease by ϕ becoming negative, the central arc sinks below the level of the adjoining branches, and the double tangents slope more and more towards its extremities, until at length they pass through the cusps; when this takes place the tangent to the second cyclode becomes perpendicular to the parent cyclode, and consequently touches the originating circle so that $p = -a$,

$$i. e. \frac{a}{2} (\phi^2 - \gamma) = -a,$$

$$i. e. \left(\psi - \frac{\pi}{2} \right)^2 - \psi^2 \frac{\pi^2}{4} = 2,$$

$$i. e. \psi = \frac{2}{\pi}, \quad \gamma = \frac{\pi^2}{4} + \frac{4}{\pi^2}, \quad \tau = \frac{a}{2} \left(\frac{\pi}{2} - \frac{2}{\pi} \right)^2.$$

As γ goes on decreasing, the double tangents quit the Moorish arch altogether and connect the two infinite branches, which turn their protuberances towards each other more and more, until finally they touch and the double tangents coincide.

This happens when $\phi = -\frac{\pi}{2}$, *i. e.* $\gamma = \frac{\pi^2}{4}$.

* For of course we may write in general

$$(\phi^2 - \gamma) + ((\phi + 2i + 1)\pi)^2 - \gamma = 0,$$

i being any integer, and ϕ will give the direction of a double tangent.

As the tail goes on still to decrease, the double tangents become imaginary, the infinite branches intersect and cut out a lune, one extremity of which, the two cusps of the cyclode under consideration, and the cusp of the parent cyclode, together form a quadrangle, which continually contracts its dimension until finally it vanishes with the tail and the central arc, and the four points merge into the remarkable *round* point indicated in fig. 1, corresponding to the parabolic or transition case between the cusped and uncusped species. This paradoxical point is a mere creature of the reason, and can by no effort be made sensible to the understanding. Observe that, in this point, the curve dips its beak, so to say, into the cusp of the parent first involute, and yet touches the original circle. Professor Cayley informs me he has met with the same kind of point in an investigation into the form of the parallels to an ellipse, and proposes to call it a triangular point, as consisting of the union of a node and two cusps. At this point, in the case before us, we have

$$p = \frac{a\phi^2}{2} - a, \quad \frac{ds}{d\phi} = p + p'' = \frac{a\phi^2}{2};$$

so that, it will be observed, $\frac{d^2s}{d\phi^2}$, as well as $\frac{ds}{d\phi}$, vanishes when ϕ is made zero.

This gives me occasion to make a remark which I do not remember having seen in the text-books, viz. that for any curve, while in general $\frac{ds}{d\phi}$ indicates the existence of a cusp, this law is subject to the exception that if a succession of such derivatives

$$\frac{ds}{d\phi}, \quad \frac{d^2s}{d\phi^2}, \quad \frac{d^3s}{d\phi^3} \dots$$

all vanish simultaneously, there will not be a cusp in fact unless the last of the flush is of an *odd* order.

Fig. 5 exhibits the critical cases (1) of the double tangents in opposition, (2) on the point of quitting the central branch, (3) in coincidence. Mr. Spottiswoode informs me that this figure has not been drawn with the same attention to mechanical exactitude as the other figures of the Plate.

In fig. 5 are seen examples of the uncusped species. The Norwich spiral (of which a word or two more presently) belongs to this species, but is not drawn; its apse lies midway between the centre of the circle and the cusp of the first cyclode. In fig. 2 is seen an example of a symmetrical tricuspidal cyclode of the third order; in fig. 4, of a unicuspid alycylode of the same order, where a loop replaces the missing cusps.

To return to the Norwich spiral; its radius of curvature ρ has been shown in the preceding rule to be always equal to its radius

vector r , reckoned from the centre of the circle. Now it is easy to see that whilst $\int d\phi\rho$ represents the arc of any curve, $\int d\phi r$ will represent the corresponding arc of its first pedal; so that the spiral in question possesses the remarkable property (capable, one would think, of some practical kinematic application) that these two arcs always remain equal to each other. More generally, if $p^2 + p'^2$ *, where p is a rational integral function of ϕ , and p' its first derivative in respect to ϕ , is a perfect square, the arc of the curve and of its pedal will always remain algebraically related. Here, then, we are led to consider the possibility of satisfying this diaphantine condition for cycloides beyond the second order. At a first glance the problem might seem to be impossible. For if the condition is satisfied by $p = F\phi$, a rational integral quantic in ϕ of the order n , it obviously will be satisfied also by $F(\phi + \lambda)$, λ being an arbitrary constant; and consequently we have only $(n-1)$ and not n disposable constants (or ratios) wherewith to satisfy the n conditions involved in a function of ϕ of order $2n$ being a perfect square.

This objection, however, is only apparent, and may at once be seen so to be, at all events as regards cycloides of an even order—say, of order $2m$. For we may suppose $p = F(\phi + \lambda)(f(\phi + \lambda))^2$, a quantic of the order m in $(\phi + \lambda)^2$, then $F^2 + F'^2 = f^2 + 4(\phi + \lambda)^2 f'^2$ is a quantic of the order $2m$ in $(\phi + \lambda)^2$, and the m disposable constants in f are sufficient to make this a perfect square. Thus, then, the n conditions are not absolutely incompatible. Still the disproof of the incompatibility might *seem* to involve the necessity of F being a function of $(\phi + \lambda)^2$, *i. e.* of the cycloide being of the symmetrical kind. Moreover, if the problem be attacked by a direct exoscopic method for cycloides of the second, fourth, and sixth orders, it will be found that the only cycloides which possess the required property *are* of the symmetrical kind, viz., for the second order, $p = \frac{a}{2}(\phi^2 - 1)$, for the fourth, $p = \frac{a}{2}(\phi^2 - 4)^2$, and for the

sixth $p = \frac{a}{2}(\phi^2 - 9)^3$ †, or $p = \frac{a}{2}(\phi^2 - 9)(\phi^2 - 36)^2$. The inference, then, might appear to be almost irresistible as to the necessity of the symmetrical form holding good. But it is *not*

* It will be remembered that $r^2 = p^2 + p'^2$. I may remark incidentally that this equation enables us to extend the well-known one, $p^2 = r^2 - a^2$, applicable to the first cycloide: the general theorem which includes this as a particular case is obviously

$$p^2 = r^2 - r'^2 + r''^2 + \dots \pm a^2,$$

p being the perpendicular on the tangent of a cycloide of any order, and r, r', r'', \dots the distances of the corresponding points in the cycloide and its successive evolutes from the centre of the originating circle.

† It is very easy to see that there is always one *reducible* symmetrical

so; it is true that only cyclodes of *even* orders are reducible, *i. e.* capable of giving r as a *rational* integral function of ϕ ; but after the sixth order, *i. e.* beginning with the eighth, non-symmetrical reducible cyclodes come into existence, and, as the order rises, become infinitely more numerous than those of the symmetrical kind.

Calling $2m$ the order, every distinct mode of making the partitions of numbers expressed by the two simultaneous equations,

$$\left\{ \begin{array}{l} x_1 + x_2 + \dots + x_i = m \\ y_1 + y_2 + \dots + y_i = m \end{array} \right\},$$

where i takes all possible values, gives rise to a system of equations yielding in general many solutions; and it is only when $x_1 = y_1, x_2 = y_2, \dots, x_i = y_i$ that the solutions are of the symmetrical kind. Moreover, even in that case, in *general*, and subject only to rare cases of exception, the reducing system of equations gives two distinct groups of solutions, one corresponding to symmetrical and the other to non-symmetrical cyclodes*. This

cyclode of the order $2m$ defined by the equation

$$p = \frac{a}{\pi(2m)} (\phi^2 - m^2)^m,$$

corresponding to which

$$r = \frac{a}{\pi(2m)} (\phi^2 - m^2)^{m-1} (\phi^2 + m^2).$$

Thus, when $m=2$,

$$p = \frac{a}{24} (\phi^2 - 4)^2,$$

$$r = \frac{a}{24} (\phi^4 - 16);$$

when $\phi=0$, we have

$$p = \frac{2a}{3}, \quad p' = 0, \quad p'' = -\frac{2a}{3};$$

whence we may derive the following construction:—Draw an *uncusped* secondary cyclode with a tail equal to one-third of the radius; unwind from this a ternary cyclode beginning from the apse, which will become a cusp in the cyclode so engendered; and from this last cyclode, beginning at its cusp, again unwind a new cyclode, which will possess a *triangular* point at the apse of its atavian secondary cyclode. This will be a quartic reducible cyclode, and, as regards *form* (irrespective of position and magnitude), the only one that exists. By the way, it may be noticed that a system of coordinates consisting of the vectorial angle and angle of contingence furnishes what may be termed a *form* equation, *i. e.* one in which actual magnitude is ignored. Thus, *ex. gr.*, $\tan \theta = k \tan \phi$ is the *form* equation to a conic.

* It is to be understood that every x and y must be an *actual* integer, *zero* being for this purpose to be regarded, not as a number, but as a negation of number. Furthermore, if the x and y numbers are not only respectively equal each to each, but have all the same value (as *ex. gr.* *unity*), the corresponding system of equations become *incompatible*; or, to speak more philosophically, the order of the system becomes *zero*, which here *per contra*

wonderful theory, this outlying and unexplored region of geometry, in which the two great continents of algebra and arithmetic trend towards and come into contact at more than one point with one another, forms the subject of a communication to be brought by the author of this Note before the Mathematical Society of London, simultaneously and under the same roof with Mr. Norman Lockyer's announcement to the Royal Society of his equally, but not more surprising and certain to be prolific discovery of the sun's unsuspected chromosphere, the analogue of the ocean of forms of which the isolated power-forms $[(\phi^2 - n^2)^n]$ correspond to the piled-up rose-coloured prominences.

Athenæum Club,
November 23, 1868.

Errata in No. 243.

P. 295, footnote ‡, for finite solution read finite rotation.

— 296, for $s_1 = a\phi_2^2 + b\phi$ read $s_1 = a\frac{\phi}{2} + b\phi$.

for $p^2 + \left(\frac{dp}{d\phi}\right)r^2$ read $p^2 + \left(\frac{dp}{d\phi}\right)^2 = r^2$.

— 297, footnote, for $G = s' - s''' \dots$; $G' = s'' - s'''' \dots$ read
 $G = s - s'' + \dots$; $G' = s' - s''' \dots$

— 301, *delete* clause commencing with the words “at such points the curve” and ending with the words “points of retrocession.”

— 303, in concluding line of fourth paragraph omit the words “either as an abortive loop or”.

ought to be regarded as a number rather than as a negation of number; for the order of the system of equations is always lowered, not only by every x becoming equal to every y , but also by any number of x 's or of y 's becoming equal to each other; so that the order of the system sinking to zero, in consequence of all the x 's and all the y 's becoming equal, is only an extreme instance of this general law. If we go to the wider case of algebraical spirals, where $p = \frac{f(\phi)}{F(\phi)}$, the difference between the degrees of f and F being still an even integer $2m$, where m is positive or negative, and require $p^2 + \left(\frac{dp}{d\phi}\right)^2$ to be made a perfect square, precisely the same method of solution is applicable as when F is of the degree zero. If we call the degrees of f and ϕ κ and k respectively, so that $\kappa - q = 2m$, we have to make

$$x_1 + x_2 + \dots + x - \xi_1 - \xi_2 \dots - \xi_n = m,$$

$$y_1 + y_2 + \dots + y_\lambda - \eta_1 - \eta_2 \dots - \eta_\mu = m,$$

$\epsilon + \eta = \lambda + \mu = i$, where i takes all possible values,

$$x_1 + x_2 + \dots + x_\epsilon + y_1 + y_2 + \dots + y_\lambda = \kappa,$$

$$\xi_1 + \xi_2 + \dots + \xi_n + \eta_1 + \eta_2 + \dots + \eta_\mu = q.$$

Every such system of partitions give rise to a system of equations containing solutions of the diaphantine problem in question, *i. e.* the problem of making r a rational function of ϕ . When the degree of p in ϕ , *i. e.* $\kappa - q$ (and consequently m) is zero, the order of all the equation-systems undergoes a marked depression.

LVIII. *Notices respecting New Books.*

The Elements of Plane Geometry for the use of Schools and Colleges.
By RICHARD P. WRIGHT, formerly Teacher of Geometrical Drawing
&c. in Queenwood College, Hampshire. With a Preface by T. ARCHER
HIRST, F.R.S. &c., Professor of Mathematics in University College,
London. London: Longmans and Co. 1868 (pp. 211).

IN noticing this work we shall, in the first place, speak of it simply as a Treatise on Plane Geometry, and abstain from weighing its merits against those of any other manual. Regarded from this point of view, we may mention that it consists of four books; of which the first two are elementary, the third treats of ratio and of similar figures, the fourth of areas. The first two books are in substance very nearly the same as props. 1–34 of the 1st book, props. 1–33 of the 3rd book, and a few propositions in the 4th book of Euclid's 'Elements'; the differences, which at first sight look much greater than they really are, consist mainly in arrangement and in modes of statement and proof. For instance, the theorem "Through any three points A B C, not in the same straight line, one, and only one, circumference can always be drawn," and the problem "To describe a circle which shall pass through three given non-collinear points, P, Q, R," are neither more nor less than the theorem "One circumference of a circle cannot cut another at more than two points," and the problem "To describe a circle about a given triangle." The arrangement, however, is very different; *e. g.* the first chapter of the first book is devoted to theorems equivalent to props. 13, 14, 15 of Euclid's 1st book. But perhaps the capital difference is that Mr. Wright reserves the solution of problems to the end of the second book. It might be thought that to assume a variety of constructions in proving theorems without showing that previously proved theorems give the means of making those constructions, would lead to reasoning in a vicious circle; and of course this might easily happen. But we believe that Mr. Wright has avoided the danger, and that the deduction of his various theorems is perfectly rigorous.

The first chapter of the third book is devoted to a very elaborate discussion of the doctrine of ratio and proportion. The line taken will be most easily understood by a citation of two definitions. Ratio is defined thus:—"Between every two magnitudes A and B of the same kind, there exists a relation answering to the inquiry *how often the first contains the second*, which is called the ratio of A to B." Proportionality is thus defined:—"When two associated and variable magnitudes are so related that the ratio of *any* two values, A_1, A_2 of A, is equal to the ratio of the corresponding values, B_1, B_2 of B, these magnitudes are said to be proportional." It is tolerably plain, from the definition of ratio, that the discussion has to be preceded by an introductory discussion of commensurability and incommensurability, which, again, introduces the conception of limits.

And in consequence the chapter, whatever its merits in other respects, is exceedingly abstruse. It might also be inferred from the definition that it was intended to treat questions of proportionality through the medium of numerical measures; and, in fact, throughout the third and fourth books pure geometry is as much as possible dispensed with, and a good deal of them consists of what is more commonly termed mensuration, or the application of algebra to geometry. In short, the contents of the third and fourth books are, though very differently treated, in substance nearly the same as a few of the propositions in the second book of Euclid, the sixth book, and the propositions in mensuration deducible from them, together with the mensuration of the circle.

Of the parts of the book particularly well worth reading, we may mention the chapter on proportion, already described, and that on regular polygons; but besides these there are scattered up and down the book many new or, at least, unusual demonstrations, and several new constructions for the solution of problems. Moreover at the end of each chapter are many well-chosen *exercises*, in all between three and four hundred in number. In a word, Mr. Wright has produced a work of considerable merit, which no one could study from first to last without obtaining a thorough acquaintance with the elements of plane geometry; and as it is always instructive to see the same thing from two points of view, a student who had learned geometry from Euclid's 'Elements' would doubtless learn something from perusing it.

We have hitherto spoken of the book simply with reference to its contents. It has, however, a further claim on our attention; for it is expressly designed to supersede Euclid's 'Elements' as a text-book. In fact Professor Hirst mentions in his preface that it had its origin in an experiment made by himself some years ago in the school connected with University College, London; and the experiment was that of "teaching geometry instead of teaching Euclid." Now we are perfectly certain that any class taught geometry by Professor Hirst would be extremely well taught, and that at the least the usual percentage would leave the class with a sound knowledge of geometry whatever the text-book used, or if, as seems to have been the case, no printed text-book was used. Yet we think the antithesis hardly just. The real subject of the experiment was whether Mr. Hirst could teach geometry most conveniently by means of a manuscript of his own composition, or by using Euclid's 'Elements of Geometry' as a text-book; and the question suggested by the present work is whether, on the whole, it were better to use as a text-book of plane geometry Euclid's 'Elements' or Wright's 'Elements.' The question is fairly raised in the present case, because it is unembarrassed by the totally distinct question of alleged errors in treatment of detail.

Whether, as a matter of fact, Euclid's 'Elements' will be displaced by a variety of text-books, some used in some schools, some in others, we do not know; but we must not disguise our opinion that the

change, if effected, will be for the worse, even if books as good as Mr. Wright's are adopted. We think that an advocate for the use of Euclid's 'Elements' might fairly urge—

(1) That, on the whole, the first and third books of Euclid would prove easier to a learner than Mr. Wright's first and second books.

(2) That the arrangement by which theorems are treated first and problems afterwards is calculated to weaken the learner's perception of the conditions of proof, and that it might in many cases lead to an unnoticed failure of proof. And he might urge that most learners, particularly boys, more readily understand a reason for a thing *done*, than a reason for an abstract truth of which they do not see the use.

(3) That to look at truths so elementary as Euclid 35. I., and the propositions of Book II., through the medium of numerical measure is to take unnecessarily a very artificial view of those questions, although its artificial character may be disguised by familiarity.

(4) That, as a matter of science, Euclid's treatment of proportion has the advantage of being perfectly rigorous and extremely elementary, Euclid's definition embodying perhaps the most elementary conception that can be formed of equality of ratios.

(5) That, from a teacher's point of view, it is possible to make most boys understand the sixth book, and then to lead them on successively to geometrical limits and to numerical measures, when the question of incommensurability can be faced; but that to try to master all three difficulties at once is to undertake a nearly hopeless task; and that practically few, if any, learners would get any deeper notion of ratio than that of commensurable magnitudes, at least until they looked back to their elementary work after having pursued their studies into the higher branches of mathematics.

LIX. Intelligence and Miscellaneous Articles.

ON THE CONDUCTIVITY OF SOME LIQUIDS FOR HEAT.

BY A. PAALZOW.

AS far as I am aware, there are no statements respecting the conductivity of liquids for heat besides those of Despretz*. Occupied with investigations on the conducting-power of liquids for heat, I was interested, for theoretical reasons, with the question whether in liquids also there is a connexion between the conduction of heat and the conduction of electricity. Hence I instituted the experiments I am about to describe, only with the view of acquiring a general knowledge of the phenomenon in question, and not with the view of acquiring accurate numbers; for I did not possess the necessary means of solving such a problem.

I used for the experiments a cylindrical glass vessel of 60 millims.

* *Ann. de Chim. et de Phys.* vol. lxxi. p. 216.

internal diameter. It was filled with the liquid to be investigated; and on the top, touching the liquid, was a copper or leaden vessel, into which steam was continually passed from a small steam-boiler. The condensed water was reconducted into the steam-boiler.

Four horizontal thermometers were fixed laterally in the vessel, in such a manner that their bulbs coincided with the axis of the cylindrical vessel. Owing to changes affecting the glass vessels, I was not always able to give the fourth thermometer the same distance from the third; and I shall therefore not always give its temperature. The liquids to be investigated had always to be heated about eight or ten hours before they exhibited constant temperature.

Water was first of all compared with solution of chloride of sodium. The heating-vessel was of copper; the first thermometer was 10 millims. from the side of the vessel to be heated; the others followed at distances of 60 millims.

I. Water and chloride of sodium.

	Water.	Chloride of sodium.
1st thermometer	72°·4 C.	72°·2 C.
2nd thermometer	26	23·8
3rd thermometer	20	17·9
4th thermometer	19	17·4

External temperature 13°.

Forming, now, from the first three temperatures t_0, t_1, t_2 the quotient $q = \frac{t_0 + t_2}{t_1}$, it becomes 3·6 for water, and 3·79 for chloride-of-sodium solution, from which it follows that water conducts better than common salt.

As the thermometers were so far apart that the temperatures very rapidly decreased, the first three thermometers were fixed 30 millims. apart. Water and concentrated solution of sulphate of copper were compared.

II. Water and sulphate of copper.

	Water.	Sulphate of copper.
1st thermometer	73°·2 C.	72°·5 C.
2nd thermometer	34·3	34·2
3rd thermometer	22·2	22·2
External temperature. . . .	14·5	14·5

The quotients are 2·78 for water, 2·77 for sulphate of copper; both, therefore, conduct equally well.

For comparing water with sulphuric acid (spec. grav. 1·25), a leaden heating-vessel was used: the first thermometer was at a distance of 14 millims. from the heating leaden surface.

III. Water and sulphuric acid.

	Water.	Sulphuric acid.
1st thermometer	63° C.	65°·7 C.
2nd thermometer	31	32
3rd thermometer	22·75	23
External temperature..	16	16

The quotients q are 2·74 for water, 2·77 for sulphuric acid ; so that water conducts a little better than sulphuric acid.

In order to compare also a more concentrated solution of sulphate of zinc with water, I put the apparatus together again, and investigated mercury at the same time.

IV. Mercury, water, sulphate of zinc.

1st thermometer	82° C.	68° C.	65° C
2nd thermometer	65	36·2	32·5
3rd thermometer	50·8	25	20·2
External temperature..	16	14	15

The quotients (q) are 2·04 for mercury, 2·57 for water, and 2·64 for sulphate of zinc.

To calculate the ratios of the conducting-powers of different liquids, the quotients

$$q = \frac{t_0 + t_2}{t_1}, \quad q_1 = \frac{t_1 + t_3}{t_2}$$

should have been constant, which, however, is not the case, as the first Table shows. At any rate, a greater number of equally distant thermometers should have been used as a control.

I content myself, in conclusion, with arranging the liquids in series in which the best-conducting precedes.

Conducting-power for—

Electricity.	Heat.
Mercury.	Mercury.
Sulphuric acid.	Water.
Chloride of sodium.	Sulphate of copper.
Sulphate of zinc.	Sulphuric acid.
Sulphate of copper.	Sulphate of zinc.
Water.	Chloride of sodium.

Poggendorff's *Annalen*, No. 8, 1868.

MOLECULAR CHANGE PRODUCED BY MAGNETISM IN STEEL BARS. BY M. TRÈVES.

More than twenty years ago several physicists, among others

Messrs. Wheatstone, Joule, Wertheim, and De la Rive, showed in various ways the influence of magnetization on the molecular constitution of iron and of steel.

The vibrations produced in rods and wires of steel or of soft iron by an alternate succession of magnetizations and demagnetizations, as well as the modifications which magnetization produces in the elasticity and even the dimensions of these metals, have led to the assumption that the development of magnetism is due to a common direction imparted to the particles of magnetic metals, and consequently to a change in the arrangement of the molecules—a conclusion directly verified by a great number of researches, and especially by those of M. Wertheim. The following new fact confirms in a remarkable manner this mode of view.

M. Trèves has been led to examine the change of molecular condition produced in a bar of iron when it is subjected to a magnetic action.

He takes two identical steel tuning-forks, which therefore are in unison; he provides them with mirrors by Lissajou's method, and arranges them opposite and in planes at right angles to each other. One of them is surrounded by a stout helix, through which is passed the current of a battery of eight elements. If one of them be sounded by a bow the vibrations commence; and as the tuning-forks are in tune, the figure reflected in the second mirror is a perfectly fixed luminous circle. Magnetizing suddenly the tuning-fork in the middle of the coil, the luminous circle is at once inclined, changes into an ellipse, vibrates right and left with a velocity which gives the measurement of the new vibratory motion.

The tuning-forks used gave 136 simple vibrations in a second; and it has constantly been observed that a beat was produced at intervals of six seconds, which means that in a second the tuning-fork suddenly magnetized gave $136\frac{1}{6}$ vibrations. If the current be opened (that is, the tuning-fork restored to its natural state), the figure ceases to move and again becomes a fixed circle.

Since 1847 Mr. Joule*, working with soft iron and with steel wires stretched or not stretched, had observed the changes in length and volume when they are magnetized. The preceding arrangement may also be utilized for this investigation; and M. Trèves proposes to use this optical method, as well as graphically tracing vibrations, to observe these effects in steel according to their composition and physical state.—*Bibliothèque Universelle*, September 15, 1868, *Comptes Rendus*, August 3, 1868.

* Philosophical Magazine, S. 3, vol. xxx. pp. 76, 225.

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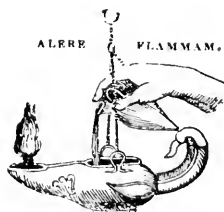
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